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MAN-POWER FOR HIGHER ADMINISTRATION

SIR GODFREY INCE, permanent secretary of the Ministry of Labour, emphasized in a statement in London on January 9 that Britain faces a shortage in man-power likely to extend over several years. A growing appreciation of this fact is to some extent behind the concern which has been recently expressed, not merely at the absolute size of the Civil Service, but also at the relative proportion of the nation's man-power employed in the national and local government services. A total of more than two millions employed in such services and a ratio of one Civil servant to slightly fewer than three persons employed in the whole of the manufacturing industries of the country consorts ill with the over-riding need for increased production in almost every field. While not all the changes involved in the nationalization proposals of the Government affect appreciably the proportion of persons employed in production to those employed in distributive, clerical or administrative work, there are undoubtedly genuine misgivings on the part of impartial observers, as a House of Lords debate on December 18 testified, about the pace at which that programme is being executed and the consequent strain imposed upon the Civil Service.

Nor is this all. While some eight and a half per cent of the working population are engaged in local or national government service, the administrative class, on whom the main burden falls, has only increased to about 4,000, or double the numbers on April 1, 1939. It was argued by Lord Schuster that it is impossible from the reservoir of strength which we now possess to recruit sufficient people to the Civil Service fit to bear the responsibilities and to discharge the burdens which at present confront us. That point of view was not accepted by Lord Ammon, but neither he nor any other Government spokesman appears to appreciate the real dangers of over-straining the senior administrative officers of the Service. Under such conditions, it is idle to expect the constant scrutiny of methods and procedure necessary to eliminate redundant work and obsolete practice or controls which is essential to secure the utmost economy and efficiency in the use of staff.

Reports from the Select Committee on National Expenditure repeatedly stressed this point during the War; but while it is probably impracticable to increase very rapidly the administrative class of the Civil Service, too little attention is commonly given in discussions of the man-power situation to the way in which a shortage of persons qualified to fill professional and administrative posts may seriously impede the nation's recovery and its economic and social progress. The Barlow Committee on Scientific Man-power recognized that some order of priorities must be laid down; but neither in that Report, nor in the observations on it which the Parliamentary and Scientific Committee has issued under the title "Universities and the Increase of Scientific Man-power", is sufficient attention paid to the bearing

of efficient management on the wise use of man-power. Its importance is well brought out in a broadsheet, "Britain's Need for Brain-power", in which *Planning* points out how efforts to increase the efficiency of industry and to develop the health, education and other social services are creating a demand for men and women with professional and administrative training much greater than could be met even if the pre-war level of recruitment had been maintained, and there had been no seven years gap in recruitment to some professions, as well as a drastic reduction in the number of those passing through the universities.

In such circumstances, it is essential not only to increase the supply of trained man-power as rapidly as possible and to see that qualitatively the increased supply is related to demand, but also to avoid so far as possible a wasteful scramble for the limited resources immediately available. Industry has already recognized that it would be unwise to attempt to attract immediately too large a proportion of scientific workers for the various research posts in the post-war development schemes planned; and the Government might well give more evidence of a similar wise restraint, and adjust the tempo of its plans for expansion of the research associations, for example, and for social services and nationalization, to the size of the scientific or administrative staff which can reasonably be expected to be available. So far there has been little sign that any attempt has been made to plan the use of Great Britain's limited resources of trained man-power to the best advantage, apart from the broad assessment of priorities in the Barlow Report.

While the *Planning* broadsheet stresses the need for such planning, it represents chiefly a careful survey of the future requirements of the professions and the upper ranks of industry, commerce and the public service, and of the arrangements desirable for recruitment. It is not easy to forecast the extent of the future demand of the Civil Service for trained men and women, and the figures circulated by the Chancellor of the Exchequer in Hansard of December 17 give little guidance. As already noted, the administrative class of 4,000 could probably with advantage be expanded, but even at 6,000—which is three times the pre-war level—would only provide a career for a comparatively small proportion of a doubled output of graduates. Some arts graduates may well find their way into the executive class, now numbering about 44,000 as against 19,000 in 1939; but the bulk of the 46,000 university students at present reading subjects other than pure or applied science must look elsewhere than to the Civil Service for careers.

On the other hand, the professional, technical and scientific full-time staff in the Civil Service numbered approximately 41,000 on October 1, as against 32,000 on July 1, 1945, and only 11,000 in 1939. Disregarding any such staff in the unanalysed total of 73,000 part-time workers on October 1, it would seem that the Civil Service is likely to absorb in future a considerably greater proportion than corresponds with the fifty per cent increase in scientific students at present in the universities, or than the hundred per cent increase contemplated, according to the Barlow Report, in university expansion during the next five years. When all allowance has been made for the elimination of unnecessary controls and for the improvement of operating methods and economy of man-power, the higher ranks of the Civil Service on both the scientific and non-scientific sides may fairly be expected to take rather more than a *pro rata* proportion of the increased output of graduates that would result from doubling the numbers coming from the universities.

It is, of course, much more difficult to estimate exactly the extent of the increased demand for arts graduates than that for graduates in science and technology. More graduates will be needed for local government services, for social work, for law and accountancy, and probably also for journalism, broadcasting, the theatre and the cinema; but whether industry and commerce will in fact take the greater number of arts graduates that they could, and should, is less certain.

Emphasizing that the community needs more men and women with the broad vision and lively critical faculty which a liberal education is designed to foster, *Planning* suggests that the output of arts graduates might be increased gradually to double the pre-war numbers by 1955, whereas that of science graduates, as the Barlow Committee concludes, should be doubled as soon as possible. For technologists, however, *Planning* is inclined to query the advisability of doubling the number. In some professions, such as medicine, an increase of about 20 per cent should be sufficient, while in others, such as dentistry, more than doubling has been recommended by the Teviot Committee.

Reviewing the professions generally, the *Planning* broadsheet points out that those for which forecasts have been made fall into three groups. First, there are those such as teaching, nursing and dentistry, in which a substantial increase in total strength is required and an increased annual intake throughout the years 1946–55. Next there are those such as accountants, company secretaries and librarians, in which the pre-war annual intake will probably be sufficient, once the fall in recruitment during the War has been made good. The third group comprises those professions, such as opticians, pharmacists and, possibly, solicitors, in which the total number is now sufficient and in which a smaller annual recruitment than before the War will be adequate.

One attempt to provide such a forecast has been made by the Manchester Joint Research Council. A survey of the Greater Manchester area attempts to assess the increase to be expected in the number of scientifically trained men employed by industry in the coming years as compared with 1939; it represents, in fact, something of the type of report on trends of employment which the Hankey Committee on Higher Appointments recommended should be issued from time to time by the Appointments Department of the Ministry of Labour. The university appointments boards might have undertaken similar surveys, but apart from that of the Cambridge

University Appointments Board on "University Education and Business" (see *Nature*, 156, 671; 1945), nothing has appeared. The Manchester Joint Research Council's report does, however, provide some means of gauging the extent to which recent developments in management, such as the formation of a Central Institute of Management, are likely to stimulate the demand for better trained men. The report shows that the greatest percentage increases in the number of firms employing graduates compared with 1939 were in textile manufacture (62 as against an overall of 32), engineering (35), chemicals (21), and textile finishing (18). The demand for graduates in industry is estimated at 80 per cent above the pre-war figures, and firms employing 85 per cent of these graduates expressed a preference for men who have had experience of research before entering industry. The biggest increase estimated was 100 per cent in chemicals, with 62 and 60 per cent in engineering and textile manufacturing, respectively. In more than 60 per cent of the firms offering employment to graduates, the higher executive posts are open to any scientific worker who shows himself to be competent and adaptable. More than 80 per cent of such firms have openings in plant control—works manager grades—and 55 per cent have a research and development department. Generally, firms employing large numbers of graduates prefer men with experience of research methods, whereas the smaller firms lay greater stress on technical training for graduate staff. While there is a sharp increase in the anticipated employment of graduates, the increase in the number of firms proposing to employ them is less marked, and it would appear that firms already employing them are readier to increase the employment of graduates than those who have not yet done so. The increase in the employment of graduates in scientific and technical capacities might have been expected, but that on the managerial side could not have been assumed with the same confidence.

It is essential to have more surveys of this kind and also to devise some system of forecasting future requirements. Means must also be found of ensuring that effect is given to the findings of such forecasts, so that we may be able to secure the fullest development and employment of the nation's reserves of brain-power. Both university expansion and the programme of nationalization, so far as they affect the Civil Service and demand the services of trained managers and administrators, must conform to, and be in step with, such findings.

Such forecasts of future requirements must form the basis of any recruitment policy; but once again *Planning* points out that Britain has the additional responsibility of training highly qualified men and women for service abroad. This applies not merely to administrators and business men but also to scientific workers, engineers and agricultural experts, who in recent years have gone out in increasing numbers to less highly industrialized countries. Moreover, it is important that university and professional training should be available in Britain to men and women from the Dominions and Colonies, and from foreign countries also. Further, it is essential to avoid regarding the problem exclusively from the demand side. A university education should provide not so much a professional technique as an intellectual training which is valuable in any walk of life. Training is not necessarily wasted when a man leaves the profession for which he has been trained, and it is important that allowance be made for transfers from one profession to another, and from a specialist to an administrative post.

DIAGNOSIS OF A FAILURE

Religion and Science

A Diagnosis. By the Rev. Dr. Charles E. Raven. (L. T. Hobhouse Memorial Trust Lecture No. 16, delivered on 1 May, 1946, at Bedford College.) Pp. 16. (Oxford: At the University Press, 1946.) 2*s*. net.

A RIGHT understanding of the causes and history of the conflict between modern science and the Christian religion is of primary importance for sociology to-day. It seems self-evident that scientific development has outrun ethical progress, and the consequences of this may be disastrous, though the situation admits of no simple remedy. The trouble is that though science can get along well enough in isolation from metaphysics, this is not equally the case with ethics, which seems to require for stability some sort of religious or philosophical background. The traditional European ethic has hitherto been based upon the Christian religion, and the decay of faith in that religion has been accompanied by signs of decay in the associated ethic.

Decay of faith in a religion may be due to some radical and incurable flaw such as the intellectual untenability of its fundamental tenets. Or, on the other hand, it may be due to faulty presentation of these beliefs, or to some twist or perversion which they suffered in the past but which might now be remedied.

Canon Raven takes this latter view, namely, that the present divorce between modern science and the Christian religion—a divorce fraught with serious consequences—is due to a twist of long standing, whereby a religion which originally recognized the worth and significance of Nature and history, came to adopt an attitude of contempt towards both. In his view the factor responsible for this perversion was the nature of the world into which Christianity was born. Its disillusion, corruption and brutality warped the new religion's development. There developed a tendency to contrast the supernatural with the commonplace; the wholesomeness of the Christian ethic became tainted with asceticism; eternal life became identified with the hereafter rather than with the here and now. God became again King and Judge rather than Father; the divinity and humanity of Christ were regarded as mutually incompatible, leading to the theological bankruptcy of Chalcedon. As for the creative conception of the Holy Spirit, this aspect of the divine nature was, so to speak, put into commission and became little more than a synonym for the supernatural prerogatives possessed and bestowed by the clergy. Thus between the ethos of the Gospels and that of the Creeds, there came about a change not only of climate but also of basic principle.

"The Gospels accept and sanctify the world of nature and history; the Creeds tend to renounce it:

the New Testament presents Christ as the consummation of the one natural-supernatural order; theological orthodoxy reshapes him as a divine intruder sent to rescue the elect from a world inevitably doomed."

St. Augustine embodied the pessimism of the time in treatises which shaped the future, condemning Nature as totally corrupt, and expounding as his philosophy of history a rigid dualism of natural and supernatural. Reinforced by the defeat of Pelagius, and coinciding with the growth of monasticism, Augustinianism shaped the Catholicism of Europe. Even in the brilliant zenith of medieval thought in the thirteenth century, the interest in natural phenomena which found religious expression in St. Francis, influencing also the Emperor Frederick II and the great schoolman Albertus Magnus, was powerless to affect the general outlook. The Englishman Roger Bacon was practically a unique phenomenon among the faithful, and nowhere in current views of Nature is there to be found any trace of first-hand observation or factual accuracy. Even the pictures in the herbals are almost invariably copies, debased and formal, of Græco-Roman originals. Thus the medieval Church was totally unable to adjust itself to the new ideas of the Renaissance.

The situation was that the magnificent system of Catholic metaphysics developed by the Schoolmen had been combined with a physics which were hopelessly defective; and the conviction had begun to prevail that problems could no longer be solved by appeal to authority, but that there was need for first-hand observation supplemented by experiment and formulated as hypothesis. The scientific movement, of which the foundation of the Royal Society in 1662 was a crucial point, reasserted the importance for philosophy and for religion of a sound and serious study of Nature and history. In the great days of the seventeenth century, there was a brilliant but brief period in which a new and satisfying interpretation of religion—a Christian synthesis worthy to replace the scholasticism of the Middle Ages—was foreshadowed and almost attained. Sir Thomas Browne in his "Religio Medici" and the Cambridge Platonists, Benjamin Whichcote, John Smith and others, expounded and developed it.

Yet the promise of this movement was not fulfilled, and Canon Raven examines the causes of failure. The scientific interest of the seventeenth century was predominantly biological in character. It was the study of life and the work of zoologists, botanists, physiologists and medical men. Thus the abstractness of the scientific outlook, its estrangement from life and culture, and its concentration upon mathematics, were by no means in evidence. Only with the coming of Newton did the narrowing of the field take place, and the Cartesian doctrine that science is mechanistic begin to prevail. Doubtless biological studies had reached a point when their further development must await progress in physics and chemistry, and it was perhaps inevitable that the Cartesian compromise (which was really a surrender) should be accepted, namely, that science should be content with the sphere of the material and that which is capable of exact measurement, while religion and the qualitative aspects of existence should lie outside its sphere.

What actually took place was a complete reversal of the medieval achievement. Then an ambitious metaphysical system was associated with grossly defective theories of the physical world. But now increasingly complete knowledge and control of the physical is associated with a metaphysic inadequate to the interpretation of life. Man's religious and moral experience is dismissed as illusory. Our development, like that of the medieval epoch, is lopsided, though our equilibrium wavers in an opposite direction.

Such is Canon Raven's diagnosis of the failure of our time to bring religion and science into line. Whether the new synthesis will come in time to save civilization from disaster remains to be seen.

J. C. HARDWICK

BREEDING BIRDS OF DENMARK

The Breeding Birds of Denmark

With Special Reference to Changes during the Last Century. By Poul Jespersen. (Published by the Danish Section of the International Committee for Bird Preservation.) Pp. 79. (Copenhagen: Einar Munksgaard, 1946.) 6 Cr.

THIS small book is a brief account of the changes which have taken place in the breeding birds of Denmark during the last century and a half.

The first part is a systematic list of all the birds which nest or are said to have nested in Denmark during the period under review. There are a few remarks on their increase and decrease with suggestions as to the causes. The second part is devoted to lists of species which have changed their status or are sporadic or occasional breeders. Finally, there are a few pages on the distribution of a number of birds the nesting of which is restricted to certain parts of the area.

Of the 217 species said to have bred in Denmark, thirty must be eliminated, since the evidence is not above suspicion. These include the willow-tit, grasshopper-warbler, dipper, fieldfare, golden eagle and jack snipe. Such species as the roller, hoopoe and the larger birds of prey ceased to nest during the nineteenth century, and the evidence points to the fact that this was due to man's direct interference. More recently seventeen other birds have seriously decreased, and among them five birds of prey, some of which, no doubt, as the result of man's persecution, though in the case of the marsh harrier draining of breeding sites is the main cause. This, too, has affected the bittern, wood sandpiper and black tern, while the planting of heaths has restricted the nesting sites of the black-game and golden plover. The green woodpecker is included in this list and in earlier times was generally distributed, but is now restricted to parts of Jutland, where, however, it appears to be spreading, so the decrease may be merely temporary.

The white stork, which the fairy tales of Hans Andersen has made everyone associate with Denmark, is still common in many parts of Jutland, though seriously diminished in numbers. At one time there were two thousand breeding pairs in Denmark, but by 1930 these had diminished to 250. No one cause can be assigned for this decrease; but fortunately the picture is not quite so black as this suggests, since in recent years the numbers have increased and there are said to be 500 occupied nests.

The planting of large tracts has led to the increase of some birds, especially tits. The cole-tit, formerly known only as a winter visitor, is now a common

nesting species, having spread from Germany. The Central European race of the crested tit, *Parus cristatus mitratus*, was first found breeding in Jutland in 1890 and has since extended its range. It is reported nesting from N.E. Zealand, but whether the same race or the typical one is not known. The colonization of Denmark, however, by two separated races is known in the case of the bullfinch. The small European race, *P.p. minor*, began to spread into Denmark from the south some twenty years ago and nests in Jutland and Funen. In 1916 the northern bullfinch, *P.p. pyrrhula*, bred in Bornholm and in 1934 in Zealand. The spread of these two forms will be watched with interest.

Mr. Poul Jespersen has done his work well, and the Danish Section of the International Committee for Bird Preservation is responsible for the publication. If other sections of the International Committee would publish similar accounts they would be of great interest.

N. B. KINNEAR

SOCIAL BIOLOGY FOR SIXTH FORMS

An Introduction to Social Biology

By Alan Dale. Pp. vii+396. (London: William Heinemann (Medical Books), Ltd., 1946.) 15*s.* net.

THERE has been a marked tendency in recent years for educationists to review the contents of school curricula so as to bring them into closer relationship with everyday life. Biology, in particular, has been the target of much criticism on the ground that most school teaching was but a faint and distant reflexion of advanced university courses—or, as one critic put it, "the beverage of school children is Heintschel and Cook and water". Signs have not been lacking, however, that changes are on the way. Some School Certificate biology syllabuses have been 'humanized', the journal *Biology* has added "*and Human Affairs*" to its title; the columns of *Nature* have had articles discussing the significance of social biology and proposing the establishment of a National Institute of Social Biology; and now the social biology text-books are beginning to appear.

Mr. Dale's selection and treatment of material will meet with general approval. His main 'type' species is *Homo sapiens*; but he avoids the pitfall of narrowing biology into human anatomy and physiology. He gives much attention to the very important matters of sex and reproduction, but avoids the error of making 'social biology' a euphemism for 'sex instruction'. He discusses many aspects of human health and disease, but steers clear of morbid pathology. He relates the social life of humans to that of other animals, but avoids the temptation to make of man a magnified ant. In short, his material is of intrinsic interest and of social significance, and is built up into a systematic and scientific course of biology.

Many readers, however, will object on philosophical grounds to phrases such as "matter imbued with life", and to conceptions such as "a centralised pool of life, to which all life returns after the death of the host-matter" (p. 2). The final chapter on "The Nature of Life" does, however, present some alternative philosophical conceptions, although it makes no mention of the approach of dialectical materialism. Most readers, one hopes, will now be sufficiently aware of the harm that has been done by race theories to regret the use of the phrase "negroid and australoid races" (p. 42); while it is perhaps something of an exaggeration to assert that "the phenomenon of sex has basically nothing to do with reproduction" (p. 99). It is also somewhat surprising that a discussion of economic–demographic problems leads to the conclusion that "The ideal would be to have the bigger families born to parents of the upper classes and the smaller families in the lower classes", rather than that wealth should be redistributed.

Despite these and other criticisms, however, this is a book to be welcomed. It is well written, attractively produced and excellently illustrated—and it should go far to answer those critics who have claimed that courses in social biology would be merely 'soft options' for those who could not face the more academic courses.

CYRIL BIBBY

LIFE OF SPINOZA

Spinoza

Portrait of a Spiritual Hero. By Rudolf Kayser. Translated by Amy Allen and Maxim Newmark. Pp. xix+326. (New York: Philosophical Library, Inc., 1946.) 3·75 dollars.

THE author of this book allows himself the liberties of the novelist. He is omniscient and omnipresent. He knows Spinoza's private thoughts and how he looked on occasions when he was quite alone. No questions about authenticity of evidence trouble him. All this would have mattered little had he possessed the imaginative insight and power of expression which can justify such flouting of the rules. As it is, the book is neither reliable biography nor a living picture of a personality. It is not without value. Spinoza's stature is so great that it would need a very bad writer to shut him out of sight altogether. Mr. Kayser gives a great deal of useful information about the circumstances of Spinoza's life and about his contemporaries. The excursions into legend do not seriously affect the general tenor of the narrative, though they do throw doubt on the author's critical capacity.

Our main source of information about Spinoza is the short biography by Colerus, which occupies, in the English version and apart from polemical passages, barely twenty pages in the late Sir Frederick Pollock's book. As a good Lutheran, Colerus is shocked at Spinoza's heresies and says so explicitly and in no measured terms. But as an honest man who knew Spinoza personally he is equally shocked at the silly malicious gossip about him, and he writes in order to stop slanderous tongues from wagging. They went on wagging for about a century, but they stopped at last, and now his testimony remains as almost the only valid contemporary record. Colerus was not an artist in words and was something of a pedant, like many other Germans. But he had positive qualities not too common among people of any nation; he loved the truth, and knew a good man when he saw him. Thus we can see a great deal of the personality of Spinoza through his bald sentences. Spinoza is a supreme example of honesty and courage; the perfect model of the wise man of Stoic theory, who really lives up to his philosophy. He shows that it is possible, by accepting all events equally as inevit-

able, for a man to be so indifferent to the worst that circumstance or human malice can do, that in effect he becomes the master of circumstance and malice is disarmed against him. Spinoza is also the only philosopher known to history who earned an honest living by the work of his hands. A. D. RITCHIE

ADVANCED THEORY OF STATISTICS

The Advanced Theory of Statistics
By Maurice G. Kendall. Vol. 2. Pp. vii+521. (London: Charles Griffin and Co., Ltd., 1946.) 50*s*. net.

THE first volume of this work, reviewed by Prof. R. A. Fisher in *Nature* of October 16, 1943, covered the subjects of frequency distributions and their properties; theories of probability and sampling and the sampling distributions to which they lead; and the theory of correlation. Chapter 12 on the χ^2 distribution was described by the author as something of an intrusion. A new edition of volume 1 in 1945 had no alterations of substance. The present very welcome volume 2 falls into five sections, dealing with estimation, the theory of statistical tests including the analysis of variance, regression analysis, the design of statistical inquiries, and the analysis of time series.

Prof. A. D. Ritchie, in *Nature* of November 2, 1946, p. 639, has drawn a distinction between the man of science and the technologist which can, I think, be applied, with a difference, to mathematicians. Some, if they carry out calculations, do so as practice for the sake of understanding. Others, though they must understand first, do so for the sake of the practical application. I may perhaps speak for those who want to apply statistical methods in various fields—in my case experimental psychology and education—but want also to understand them, indeed become interested in them for their own sake. Such must welcome Mr. Kendall's book warmly, and perhaps especially this second volume, in which Chapters 23 and 24 on the analysis of variance, and 25 on the design of sampling inquiries, are of peculiar interest to them.

In agricultural experiments, properly designed, there seldom arises the problem of "unequal numbers in subclasses" (par. 24.6 *et seq.*). But in all sociological experiments, numbers in subclasses are liable to inequality. Absences from school affect one class more than another. An island population does not fall into occupational or racial classes of equal size. So the procedures to be adopted with 'non-orthogonal' data (pp. 220–9) are of importance. I am sorry there is only a page and a half (pp. 262–3) about 'confounding'. It is very useful, however, to have everything in one book, and the examples and exercises (easy and difficult) are an admirable training and clarification.

Mr. Kendall describes the work on canonical correlations of Hotelling and others, on pp. 348–58. Their perusal reminded me of a problem which I posed a few years ago, to which I have not received an answer. In work with intelligence tests the canonical correlation technique appears as the problem of finding sets of weights to apply to two batteries of tests so that the correlation coefficient of the two weighted scores is a maximum. I applied this in 1940 (*Brit. J. Psychol.*, **30**, 358) to the problem of finding what weights would maximize a battery's reliability, that is, its correlation with a duplicate battery. These weights will, however, be different from those which cause this battery to give the best prediction of some 'criterion', such as future success in a secondary school, and straining after reliability may spoil prediction, and vice versa. The problem was to find weights which would make reliability equal to prediction and both as large as possible under this condition. In simple cases a solution can be found. But what is the general solution?

Students who are not primarily mathematicians very much need and welcome preliminary general explanations of the logic and purpose of the procedures to be proved and used in the succeeding pages, and some of them find geometrical analogies helpful. Given such assistance, they are able to follow the algebraic work more easily, or at least to appreciate its general trend, and to feel able to accept, not quite blindly, the formulæ arrived at.

Such assistance is not wanting in Mr. Kendall's volume—I may instance pp. 269–72 with the diagram, at the outset of the discussion of the general theory of significance tests, and the diagrams of confidence belts on pages 68, 69 and 77. In the latter I suggest the addition in dotted lines of the whole sausage-shaped region of acceptance, of which the shaded area is a section.

The theory of probability has always been an arena for controversies and even quarrels. Mr. Kendall has refused to take sides in present-day disputes, but has objectively described both points of view. About the different approaches to the treatment of confidence intervals and fiducial inference, differences which so easily escape from one's mind even when one has again and again grasped them fleetingly, he has written two admirably lucid chapters, and has had them read by Prof. Egon Pearson and Prof. R. A. Fisher respectively to ensure that he has at least not misrepresented them. To the outsider, now one and now the other view seems the better. In sociological work, it is important not merely to consider the possibility of a hypothesis being the true explanation, but also its relative standing when compared with alternatives. Neyman and Pearson make just this point, and in addition to defining regions within which we reject a hypothesis because the odds are so and so that the data did not arise from it (of which regions there may be many) they choose among them that region which minimizes the chance of accepting the hypothesis when it is false.

On the other hand, the Behren's test does not hold in the theory of confidence intervals, and the reply that "some questions may not have an answer" seems scarcely sufficient. Then Kendall protests that we can never test a fiducial distribution by drawing samples. But we can set up some apparatus corresponding to the value of θ and find experimentally the proportion of samples which give rise to t. Then we can change θ and repeat; and so on. Surely this is an experimental frequency distribution.

Many will look forward to the future publication which Mr. Kendall half promises, in which he hopes to show that all these disagreements are more apparent than real.

The book is beautifully printed and the proof reading has been excellent.

GODFREY THOMSON

BIOCHEMICAL RESEARCH AT OXFORD UPON MUSTARD GAS

By Prof. R. A. PETERS, F.R.S.

CONCURRENTLY with the research upon arsenical vesicants which led to *BAL*[1], a biochemical attempt was initiated at the outbreak of the War in 1939 by the writer and directed in the Biochemical Department, Oxford, for the Ministry of Supply, with the object of finding either a better compound to act as a 'chaser' to follow quickly and to remove unreacted mustard gas in the skin, or an antidote capable of reversing the combination of mustard gas with the skin constituent. This review deals with the salient features of the work of the Oxford sub-group (J. St. L. Philpot, A. G. Ogston, E. R. Holiday and L. A. Stocken); for other related work see the recent reviews of M. Dixon and D. M. Needham[2], and of A. Gillman and F. S. Phillips[3].

Whatever view be taken as to the physiological and pathological changes involved in vesication, the first step is a biochemical change, better called a 'biochemical lesion'. Some indication of our attitude to earlier hypotheses of the action of mustard gas is necessary to explain our mode of attack. Two outstanding points in regard to its action upon skin are: (*a*) the minute amounts which will cause erythema and vesication; and (*b*) the delay in appearance of erythema and other pathological signs.

Much earlier (1923–25) Peters and Walker[4,5] had rejected both the theory that mustard gas poisoned by liberation of intracellular acid, and also the 'sulphone' hypothesis based by Flury and Wieland—with little evidence—upon the idea that mustard gas was first converted into dichlorodiethylsulphone by oxidation in the skin. Peters and Walker proved that, when suitably applied, dichlorodiethylsulphone was as vesicant as mustard gas, and the Chemical Defence Experimental Station at Porton showed that it had a high toxicity. But the sulphone hypothesis was nevertheless untenable for at least two reasons: (*a*) because no dichlorodiethylsulphone could be recovered from skin treated with mustard gas; and (*b*) because mustard gas failed to abolish the fixed—SH reaction in skin tissue, which rapidly vanished upon treating with the mustard gas sulphone *in vitro*. Applying the work of Hopkins and Dixon[6] upon the fixed —SH group in muscle tissue, they also found that other skin vesicants, diphenylchlorarsine and allyl isothiocyanate, abolished the nitroprusside reaction given by the fixed —SH group in skin, muscle and tracheal tissue.

No support could then be obtained for a third hypothesis, namely, that mustard gas attacked an enzyme; later, Peters[7] revived the enzyme hypothesis by showing that dichlorodiethylsulphone (as well as arsenite) had a selective action upon the brain pyruvate oxidase system, resembling that produced by aneurin deficiency; in particular, the oxidation of lactate was inhibited selectively at the pyruvate stage by concentrations of dichlorodiethylsulphone (0·087m*M*.) which left (by inference) the cytochrome and lactate dehydrogenase systems intact. Since the action resembled iodoacetate, he was led to suggest that the vesicant action was due to attack upon the same enzyme. This work was carried out independently and about the same time as work in relation to tumour glycolysis by Berenblum, Kendall and Orr[8], who found a more marked action upon glycolysis than upon respiration *in vitro* with mustard gas, dichlorodiethylsulphone and cantharidin; they did not relate this to individual enzymes.

Our work in the War started with the idea that the pyruvate system was specifically poisoned; though an N-group could not then be absolutely excluded, and in spite of earlier contrary evidence, it was thought that, as with the arsenicals, the site of attack must be an —SH group, because mustard gas so readily attacked simple thiols. It was clear that knowledge of the biochemical components affected, and of the chemical reactions involved, was essential.

Rational progress was soon made upon the mechanism of chemical action of mustard gas (Holiday, Ogston, Philpot and Stocken[9]; earlier findings upon the kinetics of hydrolysis at physiological *p*H (Peters and Walker[4]) were interpreted upon Ingold's theories (see Watson, H. B.[10]) as indicative of an SN_1 reaction; it was realized that the compound was a remarkable alkylating reagent, active at room temperature; by a generalization known as the 'competitive theory', the groups most likely to be selected for attack by replacement reactions in a mixture could be predicted (Ogston[11]). The competition factor (*F*) of a reagent was defined as the ratio of the rate of its reaction to that of water, when the concentration of the reagent is 1 equivalent per litre; the reaction with water is:

$$S(C_2H_4Cl)_2 + 2H_2O = S(C_2H_4OH)_2 + 2\,HCl.$$

Competition Factors

Thiophosphate	7×10^4	*BAL* $7{\cdot}8 \times 10^3$
Thiophosphonates (di)	$1{\cdot}0 \times 10^4$–10^5	amino $1{\cdot}8 \times 10^1$
Thiosulphate	$2{\cdot}7 \times 10^4$	carbonyl $1{\cdot}0 \times 10^1$
Diethyldithiocarbamate	$3{\cdot}4 \times 10^4$	phosphate $7{\cdot}5 \times 10^1$
Cysteine	$1{\cdot}2 \times 10^3$	

The table gives briefly some of the competition factors for different types of substance. (Similar work was started independently later by Linstead and colleagues in the United States.) Compounds likely to act as 'chasers' could now be developed upon a plan. It will be noticed how much more reactive were some complex thiols, for example, diethyl dithiocarbamate and the dithiophosphonates; the latter were new compounds specially developed for this research by Philpot and Stocken. Of all competitors, though of no value for skin treatment, they proved much the most promising; being comparatively non-toxic, they could be injected and were therefore suitable for dealing with mustard gas in the circulation; they could protect rats as well as the brain pyruvate system against mustard gas; later this work was intensively developed by Rydon, G. R. Cameron and others at Porton. In spite of their theoretical advantages, in practice, injection of these compounds proved to give no longer interval for treatment than existing anti-gas measures, owing largely to the rapidity of fixation of toxic amounts of mustard gas in the skin (Fell and Allsopp, 1940, G. R. Cameron, H. M. Carleton and K. Harrison, 1940; H. Cullumbine, 1942, U.S. workers) and to the speed with which mustard gas enters the circulation. In this, earlier quantitative work by Holiday and Ogston[12] in rats was confirmed and extended by the radioactive technique then being developed by Wormall and his team at Cambridge, and afterwards extensively used in the United States and in Canada. From a

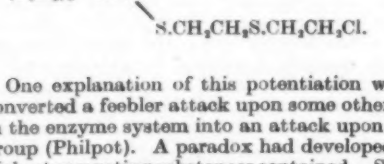
different angle Stocken[13] also proved the rapidity of mustard gas fixation in human skin by showing that his new, highly penetrant and reactive chlorinating compound (N.dichlorethane sulphonamide) was not effective against skin contamination after five minutes.

As an extension of this work, Ogston[14] made the advance for kinetic theory in preparing for the first time the half-product of hydrolysis of mustard gas, β-chloro-β′-hydroxydiethylsulphide, $ClCH_2CH_2S.CH_2CH_2OH$: (this was also done independently at Sutton Oak). By its use he found finally that the second —Cl in mustard gas hydrolyses rather more quickly than the first.

In attempts to understand mustard gas effects in the cell, the competition theory is fundamental, because it predicts what groupings might be expected to be attacked; it also shows why there must be a more general alkylation of a variety of groups with increasing concentration.

Quite early we proved that the pyruvate oxidase system was almost as sensitive to poisoning by dichlorodiethylsulphone and divinylsulphone as to arsenic (Peters and Wakelin); though there was some correlation between toxicity to this system and vesication, this did not hold for mustard gas, to which the system was less sensitive. Mustard gas was scarcely toxic to the cytochrome and succinoxidase, lactate oxidase or amino-acid oxidase systems. The pyruvate oxidase system was sufficiently poisoned by mustard gas to be used as a convenient test for protective trials with the thiol compounds (Holiday, Ogston, Peters, Philpot and Stocken[15,16]).

There was followed at this stage an interesting potentiating action of diethyldithiocarbamate upon the action of mustard gas upon the pyruvate oxidase system, by which it became practically as toxic as the sulphones and the arsenicals. Found to be a property of some dithio compounds, the action was traced mainly to the toxic action of the half-product of combination[17], namely:



One explanation of this potentiation was that it converted a feebler attack upon some other grouping in the enzyme system into an attack upon the —SH group (Philpot). A paradox had developed; all the highest competing substances contained —SH groups; the pyruvate oxidase was proved to contain —SH groups, yet our older view that mustard gas did not attack the fixed —SH group appreciably appeared to be correct. Drs. Dixon and Needham[2], and colleagues, for example, who were then developing the hexokinase theory of mustard gas and of vesicant action which appeared to cover the facts better, obtained suggestions that an —SH group was not the site of attack. This showed clearly the marked difference between the action of the arsenicals and of mustard gas.

At this period, too, a study of the alkylation of kerateine by mustard gas (Peters and Wakelin[18]) showed that though some of the thiol groups were attacked, about three-quarters of the mustard gas had combined with other groups (much more detailed studies on proteins were made by Wormall's team[19]).

Independently again, A. Pirie[5] found that collagen, comparatively free from thiol groups, combined with mustard gas. All this was evidence that, in spite of the reactivity of compounds like the thiophosphonates, the —SH groups were not so easily selected by mustard gas in the tissue. Indeed, our protective experiments *in vitro* on the pyruvate oxidase system had suggested that cysteine with comparatively low competition value was as effective in protection as highly competitive substances. For fundamental biochemical theory, these facts throw into relief the importance of understanding the reactivity of —SH groups attached to large molecules. Much evidence has now accumulated at Cambridge and in the United States that this vesicant attacks —COOH groups in proteins.

The practical failure of substances able to detoxicate mustard gas rapidly focused attention upon the possibility of removing the combined fragment. In regard to any thioether linkage formed (—C—S—C—) an interesting silver reaction was developed; if silver nitrate is added at room temperature at faintly acid *p*H to form a silver complex with some thioethers, and the *p*H then changed to faint alkalinity, the thioether may be decomposed with regeneration of the constituents (Peters and Wakelin and with Ford-Moore[3]); for example, in the case of the compound of cysteine and phenyl-β-chlorethylsulphone, the *p*H at which the splitting starts is about *p*H 7·8 (near that of the cell), and it was possible to isolate both split fragments. Curiously enough, the similar sulphide does not show this scission reaction; but it will do so if the sulphide is oxidized to the sulphoxide form. From a different aspect, similar results were reached in the United States. In regard to the labilization of a mustard gas fragment which has alkylated —COOH groups, Ogston had shown earlier that there was slow hydrolysis of the acetate at physiological *p*H, and Pirie that much of the mustard gas combined with collagen could be split in alkaline solution. It was found (Peters and Wakelin[21]; also work of Kinsey and Grant, and Bergman *et al.*[22]) that oxidation (presumably to sulphoxide) of the bound sulphur in mustard gas kerateine made the mustard gas fragment much more unstable, suggesting that much was combined with COOH. This appears to be a suggestive observation and is reminiscent of earlier biochemical work on labilizing groups in relation to enzyme action. The important point is that regeneration of cell constituents attacked by mustard gas at —COOH and —SH groups is not now absolutely inconceivable.

That poisoning of an enzyme can initiate vesication seems proved for the arsenicals; the theory of Dixon and colleagues[2] that mustard gas action is due to the initial poisoning of hexokinase is based upon its rapid poisoning of this enzyme *in vitro*. The outstanding difficulty in regard to this theory is that, *in vitro*, hexokinase is not immediately poisoned, which, though consistent with the slow appearance of erythema, is difficult to reconcile with cumulative histological evidence that mustard gas also makes an immediate attack upon skin. In searching for other interpretations, we made use of our studies on thermal burns (Beloff and Peters[23]), where we had found recently the liberation of a proteinase at an early stage from burned rat skin, owing presumably to a change in permeability. Similar experiments with mustard gas showed that within a short time of contamination there was a disappearance of proteinase, not due to inactivation by mustard gas; as in the

case of thermal burns, it seemed that this might be responsible for the epidermal separation (Beloff, Peters and Wakelin[24]). In this connexion it was most pertinent that H. Cullumbine and H. N. Ryden[25] found that our proteinase could liberate leucotaxine from some proteins and so cause vasodilatation. These experiments have led us to doubt the enzyme theory for mustard gas, in the sense that this was the first constituent attacked with the lowest toxic concentration of mustard gas, and to believe that the first attack was upon the surfaces of skin cells; though even an attack on 'surface' enzymes is not quite excluded, it is possible that changes in cell permeability with a contingent dilution of cell constituents would be dangerous. This 'cell surface' hypothesis, making the action of mustard gas somewhat analogous to that in thermal burns, was reached from a different angle by Cori *et al.*[26], and was also the view of R. K. Cannan[27].

If this be indeed the first site of attack, we are still in ignorance of the constituent concerned. During the first nine months of the War, Dr. Berenblum and Miss Schontal (Department of Pathology, Oxford) worked in parallel with our team and found that nucleoprotein in thymus extracts was powerfully and rapidly attacked by mustard gas. This would be in line with the cytotoxic action; for example, the formation of mutations (C. Auerbach and J. M. Robson[2]). At present there is a trend of biological opinion towards the idea that there is nucleoprotein in the cytoplasm; if some reactive nucleoprotein is situated in the interface, we should have a good working hypothesis for further studies. That points of fundamental scientific, in addition to practical, interest will repay further study seems reasonably certain, though they will have to be done by those who are not yet hypersensitive to its effects! A reagent capable of alkylating the nucleus at room temperature surely has great possibilities.

In conclusion, it is interesting to note that of the four biochemical lesions studied in the laboratory, two, namely those produced by aneurin deficiency and arsenicals, attack the pyruvate oxidase system. At present there is some support for the view that both heat and mustard gas appear to concentrate their initial attack upon the cell surface.

It should be recorded that a different approach was initiated by Dr. R. H. S. Thompson of this Department. In 1942[28] he found that the 'nitrogen mustards', ββ′-dichlorodiethyl-N-methylamine and trichlortriethylamine, are effective inhibitors of cholinesterase both *in vitro* and *in vivo*. Mustard gas also inhibited this enzyme but to a lesser extent. Certain of the systemic effects produced by these agents are muscarine-like in nature, so that a failure to hydrolyse liberated acetyl choline might well account, in part at least, for some of the pathological symptoms observed. But since it is clear that this is only one of several enzymic inhibitions produced by these compounds, it is not yet possible to formulate any definite relation of this inhibition of cholinesterase to the action *in vivo* of these vesicants. It may be recalled in this connexion that Cordier[29] has reported experiments upon alkaloid-like effects of mustard gas.

During the War, an independent study was made by Z. Bacq and his colleagues (see Z. M. Bacq[30]) of the effects of chemical warfare agents upon thiol compounds in relation to vesicant action.

We are indebted to the Director General of Scientific Research (Defence), Ministry of Supply, for permission to publish this work. As in the case of the work upon arsenicals, we are grateful to Mr. Davidson Pratt and his colleagues at the Ministry for their support, and to the staff of the Chemical Defence Experimental Station, Porton. We have also much appreciated discussions with Dr. Dixon and his team, as well as with others.

A reference thus, "Peters Report No. 1 (1940)", refers to a report submitted to, and given a restricted circulation by, the Chemical Defence Research Department of the Ministry of Supply in that year.

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MEDICAL ASPECTS OF MUSTARD GAS POISONING

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MUSTARD gas, or 2, 2′ dichlorodiethyl sulphide, was first prepared by Guthrie in 1859, and he commented on its vesicant properties. It is, in fact, a very versatile agent, defiling all tissues with which it comes into contact, and it is potent in the vapour as well as in the liquid phase.

Mode of Penetration of the Skin

Mustard gas penetrates the skin rapidly; but there is a latent period of some hours, under temperate climatic conditions, before any macroscopic change is produced, and there has been much speculation on what is happening to the mustard gas during that time. The problem has been studied histochemically (Cullumbine, 1944) and by the use of mustard gas containing radioactive sulphur (Moritz, Henriques *et al.*, 1942–43). It would appear that 80–90 per cent of the mustard gas penetrating the skin passes rapidly into the circulation. Of that remaining in the skin, most (about 12 per cent of that penetrating) is rapidly fixed, possibly by combination with protein, and only a very small amount remains 'free'. This 'free' mustard gas does not exist in the skin of man

except in the epidermis, and even this has disappeared within thirty minutes of contamination. No 'free' vesicant is demonstrable in the dermis even within five minutes of contamination.

The extent of vesication is proportional to the amount of 'fixed' mustard gas, and since 'free' mustard gas has only a transient life in the skin, we should expect any therapy based upon a reaction with 'free' mustard to be valueless unless applied very soon after contamination. Examples of such therapies are the reactive principles of British anti-gas ointments, which, even under temperate climatic conditions, suffer the limitation of being effective decontaminants for only a few minutes after the application of the mustard gas to the skin.

These ointments depend for their efficiency on the liberation of chlorine on the skin; but it has been shown (Cullumbine and Liddell, 1944), by histochemical means, that available or 'free' chlorine does not exist as such in the inuncted skin. The active principles of the anti-gas ointments do penetrate the skin only to be immediately reduced by the many unsaturated compounds present in the skin. Therefore, ointments depending for their efficiency on the liberation of chlorine in the skin are useless as late decontaminants for mustard gas; the only hope of preventing skin damage is by therapy which will reverse the mustard gas fixation and the physiological processes accompanying it.

Competitors

The 'fixed' mustard gas is probably held in the skin in combination with skin protein, for many workers have shown that mustard gas can react with a wide range of proteins (Berenblum, 1940; Wormall, 1940; Pirie, 1941; Peters and Wakelin, 1941; Northrop, 1942; Hellerman *et al.*, 1943; Cozan, Kenny and Grant, 1943). In fact, by substitution reactions involving the highly reactive β-chlorine atom, mustard gas can react in an aqueous phase with compounds containing the groups —SH, carboxyl, amino, hydroxyl, etc. (including proteins, amino-acids, nucleic acid, etc.), and its toxic effect has been assumed to be due to its reactivity in this fashion with one or more of the cell constituents (Ogston, 1942). Therefore, any reagent with itself an affinity for the β-chlorine atoms of mustard gas might be expected to be successful in preventing mustard gas 'fixation' or in reversing such 'fixation'. A representative selection of such compounds ('competitors') has been tested (Cullumbine and Rydon, 1944) on contaminated human skin with this end in view; they all proved less effective, under the conditions of test, than existing anti-gas ointments. The rationale of the use of 'competitors' for the treatment of mustard gas-contaminated skin assumes, of course, that the 'fixation' of the vesicant is reversible; Cullumbine (1944) and Kinney and Grant (1944), using animals and yeast cells respectively, have been unable to find any evidence for this assumption.

Leukotaxine

'Fixation' of the vesicant occurs very rapidly, but yet some hours may elapse before obvious reddening and vesication appear. This long latent period is of macroscopic significance only, and there is evidence that changes occur in the underlying tissues shortly after the mustard gas is applied. Thus Cameron and Short (1944) using rabbits, and Cullumbine (1944) in man, have shown that an increased capillary permeability occurs in the skin within a few minutes of contamination; at the same time, they have produced histological evidence of damage in the dermis and underlying structures 20–30 minutes after the application of mustard gas. The increased permeability and the early accumulation of leucocytes are apparently due to the formation of a factor similar to Menkin's 'leucotaxine'. This factor can be isolated quite early from mustard gas-contaminated skin and also from the blister fluids of several vesicants (Cullumbine and Rydon, 1943). Its formation is not specific to the action of mustard gas on the skin, since other vesicants and injuries can produce an apparently identical factor. The substance has been partially purified, and it appears to be a polypeptide of comparatively low molecular weight. Moreover, its ready production by the digestion of certain proteins suggests that it may be produced by the interaction of mustard gas and protein in the skin; the skin proteinase of Beloff and Peters (1945) may play some part in this formation.

Enzyme Systems

However, mere injection of the capillary permeability factor does not produce a vesicle. Therefore, some additional factor must be involved in the mechanism of vesication. Histological study has shown that the separation of the blister roof occurs by disruption somewhere around and along the epidermal–dermal junction. This separation probably occurs through the inhibition of certain enzymes (phosphokinases) which are necessary to cell life.

A whole host of enzymes can be inhibited by mustard gas if the conditions are sufficiently drastic, and such a general inhibition may be the cause of the necrotic action of mustard gas when applied to the skin in massive doses. However, under milder conditions, the action of the vesicant would appear to be more specific. Peters (1940) observed that the brain pyruvate oxidase system was inhibited by mustard gas and certain other vesicants, and this phenomenon has been used to measure antidotal effects too small to be detected easily *in vivo*. Dixon (1941) has also studied thoroughly a large number of enzymes. He has shown that there is a close relationship between vesication and the inhibition of glycolysis, and he has concluded that mustard gas inhibits a group of enzymes, the phosphokinases. Thus in the glycolysis system it inhibits the reactions for which the enzymes hexokinase and deutero-hexokinase are responsible. In addition, Dixon found that substances which poison hexokinase are all vesicant, and that substances which do not poison hexokinase are all non-vesicant. Again, there is a close parallelism between vesicant activity and power to inhibit hexokinase. Moreover, hexokinase has been detected in various tissues, including skin, and the visible development of the skin lesion runs parallel in time with the inhibition of glycolysis. The importance of the inhibition of the phosphokinases, and especially of hexokinase, is thus evident. Dixon, indeed, has suggested that the inhibition of the pyruvate oxidase system, noticed by Peters, may actually involve the inhibition of a phosphokinase since the oxidation of pyruvic acid appears to involve phosphorylation.

Northrop (1942) has shown that the growth of yeast can be inhibited by mustard gas without affecting respiration, glycolysis, proteolysis or invert-

ase activity. The different biological system used may account for this apparent divergence from the phosphokinase theory, though the results do suggest that even more sensitive systems than the hexokinase one may be involved.

The actual chemistry of the reaction between hexokinase and mustard gas is still unknown, and we are still ignorant of the cause of the time lag in vesication. Hexokinase is inhibited at once *in vitro* by mustard gas, but the inhibition of hexokinase in rat skin only occurs after a delay corresponding in time to the appearance of skin changes.

Sensitivity of 'Hot and Sweaty' Skins

It is a well-established fact that the skin is more sensitive to mustard gas in hot climates than in cold. Moreover, even in temperate climates, certain regions of the skin are more sensitive to mustard gas vapour than others; for example, the skin of the scrotum and of the armpits shows vesication much more easily than the skin of the rest of the body. An attempt has been made to discover the factors involved in determining the sensitivity of the skin to mustard gas vapour (Cullumbine, 1943). The experimental results suggest that the moisture on the surface of the skin plays the major part in increasing the sensitivity of 'hot and sweaty skins', although increased temperature, by increasing the solubility of the vesicant and accelerating its 'fixation' in the skin, may aid this.

Treatment of Mustard Gas Intoxication

Many methods of preventing vesication, once contamination was established, were tried with varying degree of success. One drastic but effective method was by the use of certain acids—trichloroacetic, monochloroacetic, and thioglycollic—which were effective for a considerable time after contamination and against many (if not all) vesicants (Cullumbine, 1943). These acids prevent the immediate loosening of the epidermal–dermal junction, thrombosis of the small skin vessels thus limiting the serous effusion also occurring. An intact firm epidermis instead of a vesicle is produced, though this skin may break down later.

Various local methods of treatment of the mustard gas skin lesion have been considered. The therapies have been tested by two means: (*a*) on small standard experimental burns on volunteers (Cullumbine and Sinclair, 1943); (*b*) on actual clinical cases of vesication. The findings indicate that mustard gas burns will heal in a similar manner, though possibly more slowly than thermal burns, and can be treated in the same way as thermal burns. Epidermal regeneration begins about the third day and, when complete, is often hypertrophic with ingrowths and 'nests' of epidermal cells evident in the dermis. If the blister is intact, mere collapse of the blister roof is advantageous, since pain, infection and healing-time would seem to be reduced.

It must not be forgotten, however, that general treatment is just as important in the case of mustard gas burns as with thermal burns. 'Shock', toxæmia and secondary infection must all be guarded against or treated. If the contamination has been severe, general symptoms and signs may be manifest before the skin requires treatment.

The most consistent organic lesions occurring in human cases of mustard gas intoxication are those in the intestinal tract and the bone marrow, indicating that a toxic factor must be transferred from the local site of contamination. Certainly the mustard gas, in some form or other, rapidly enters the blood from the skin (Ball, 1942; Henrique *et al.*, 1942). Lymph does not carry mustard gas at all, since the lymph draining from the contaminated hind-limb of a dog was non-toxic (Courtice, 1944). The skin itself or the œdema fluid, however, do not appear to contain a systemic toxic factor (Cullumbine, 1944). The circulating form of mustard gas is rapidly taken up from the blood by the tissues generally (Young, 1942; Ball, 1942). However, although there is no preferential absorption, some of the tissues, for example, the bone marrow and the intestinal tract, suffer specific damage, others do not (Dixon and Wormall, 1943). There is a suggestion that the toxic factors are formed locally in these specific tissues, since extracts of bone marrow from intoxicated animals will produce bone marrow changes upon injection into normal animals (Cullumbine, 1944). The damaged tissues show an early increase of capillary permeability, so that the local liberation of a capillary permeability factor may play some part in the systemic lesions. Such a factor can be demonstrated in the extracts of intoxicated bone marrow and, unlike normal serum, the serum from intoxicated animals does not inhibit the activity of this factor.

Dixon *at al.* (1943) have shown that the anaerobic glycolysis and the respiration of rabbit bone marrow are inhibited after an intravenous injection of a lethal dose of mustard gas; the action is delayed and exhibits a parallelism, in time, with the histological changes. Hence, Dixon has suggested that inhibition of hexokinase may play an important part in mustard gas systemic poisoning. It may have a part in the pathology of the bone marrow changes, but not in the intestinal changes, since Cori (1943) has shown that, after the intravenous administration of mustard gas, the hexokinase and the phospho-creatine phosphokinase of the intestine are not affected, but the inorganic pyrophosphatase and cholinesterase are inhibited.

In general, the more serious the prognosis from skin contamination the lower the white cell count. Attempts to reverse the bone marrow damage by means of non-specific stimulants have so far failed (Courtice, 1943; Cullumbine, 1943). Certain compounds with high competition factors have shown promise in reducing the mortality of contaminated animals; but the time intervals for effectiveness of these compounds are short. This suggests that, as in the skin, once 'fixation' has occurred in the tissues, the process would seem to be irreversible (Cameron and Rydon, 1944).

The mortality can also be reduced by the simple administration of sodium chloride solutions, by mouth, subcutaneously, or intraperitoneally (Cullumbine, 1944); the administration preferably commencing early and continuing throughout the possible period of mustard gas intoxication. The replacement of electrolyte lost locally and in the intestines and via the saliva, vomitus and diarrhœic stools is apparently an urgent need.

I am indebted to the Chief Scientific Officer, Ministry of Supply, for permission to publish this paper.

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EDWARD LHWYD: GEOLOGIST, BOTANIST AND PHILOLOGIST (1660-1709)

By V. A. EYLES

ACCOUNTS of the lives, work and writings of some of the more prominent naturalists and men of science who flourished during the second half of the seventeenth century and the early years of the eighteenth, among whom may be mentioned Newton, Boyle, Hooke, Ray and Plot, have provided the subject-matter of a number of books published in recent years. A further addition to these studies has now been issued as a result of the labours of the late Dr. R. T. Gunther*. It forms volume 14 in the series "Early Science in Oxford", and has been published posthumously, thanks to the co-operation of Dr. John Johnson, of the Oxford University Press.

Edward Lhwyd, the subject of this volume, was a Welshman. Though his father's name was Lloyd, he preferred to be known as Lhwyd, nearly always signing himself thus. The spelling of his name seems always to have occasioned difficulty, for he has been referred to in one place or another as Llwyd, Lhuyd, Llhuwyd, Floyd, Floid, ffloid and, *latinè*, as Luidus or Luidius.

Lhwyd was very active in his time as a student of botany and of fossils, and he also devoted much attention to philology and archæology; but he has received little mention from historians of science. Yet he was well known to contemporary scholars, among whom he was held in esteem, and with a number of whom he corresponded for many years. The deficiency in our knowledge has now been remedied by Dr. Gunther.

The opening pages of Gunther's work are occupied by a reprint of a short life of Lhwyd compiled by Richard Ellis, and published some years ago by the Honourable Society of Cymmrodorion[1]. Following this biographical account, a selection, totalling 288, from the very considerable number of letters extant that were written by or to Lhwyd has been reproduced. These are arranged in strictly chronological order, but have been divided into groups, representing successive periods in Lhwyd's life, each of which is prefaced by a commentary by Dr. Gunther. Among the correspondents John Ray and Martin Lister figure very largely, but a number of other well-known names occur in the list, including those of John Aubrey, the antiquary; Dr. John Woodward, founder of the Woodwardian chair of geology in Cambridge; Dr. Richard Richardson, the botanist; and Tancred Robinson, botanist and friend of Ray.

Lhwyd, after attending Oswestry Grammar School, went up to Oxford in 1682, being entered at Jesus College, which then, as now, had strong Welsh associations. His means were slender, necessitating his taking up a paid appointment, and about 1684 he became assistant to Dr. Plot, the first keeper of the Ashmolean Museum, with the title "Register of the Chymicall courses of ye Laboratory". He would have preferred an appointment which would have enabled him to travel abroad, so that he could acquire a wider knowledge of the subjects that interested him, but circumstances were against him. For a time, therefore, his movements were confined to journeys into the countryside within easy reach of Oxford. During these trips he searched the fields and quarries thoroughly for botanical and geological specimens, even training children and labourers to assist in the work; and he was sometimes accompanied by visiting scholars.

During these early years in Oxford, Lhwyd pursued his studies in a variety of subjects with great application, and gained the friendship of a number of the more influential members of the University. As a result, when, in 1690, Plot resigned from the keepership of the Ashmolean Museum, Lhwyd was appointed to succeed him. This position, though still not a well-paid one, gave him more freedom, and his first excursion outside the Oxford district followed before very long. It resulted from a commission to supply fresh material about Wales for a new edition of Camden's "Britannia", then being prepared under the general editorship of his friend Edmund Gibson, later Bishop of London. Lhwyd agreed to "doe all Wales; & to take a journey speedily quite through it for ten pounds in hand; and twenty copies of ye Book, when it shall be published". He visited Wales to execute this commission in 1693, and the results of his work appeared in 1695, in the new edition of the "Britannia". Lhwyd's acceptance of this work was not primarily for monetary reward; but in the hope of advancing his own reputation sufficiently to influence patrons who might finance more extensive travelling. In this he met with some success, as will be mentioned below.

Meantime, by 1695, Lhwyd's collection of fossils had grown so large that he decided to prepare a catalogue for publication. He petitioned the University to undertake this for him, but without success; and it was only with the assistance of Sir Hans Sloane, and other friends, including Sir Isaac Newton, who each agreed to subscribe for ten copies, that the catalogue ultimately appeared, in 1699, in a small edition of 120. It bore the title "Lithophylacii Britannici Ichnographia", and included brief descriptions of nearly eighteen hundred fossils, with a note of the locality in which each had been found. Many were illustrated by such carefully drawn figures that it is still possible to identify them accurately (some of Lhwyd's fossils are illustrated in Gunther's work). The book is noteworthy as the first systematic catalogue of British fossils. Of the small number printed, it is stated that thirty were sent abroad, and the work is now very rare. A reprint, with some additional matter, was published by the Clarendon Press in 1760.

Returning again to 1695, Lhwyd's contribution to Camden's "Britannia" was evidently favourably received. Gibson, in writing to a correspondent in Oxford, remarked: "When I tell you, that the whole business of Wales was committed to the care of Mr. Edward Llwhyd . . . no one ought to dispute the justness and accuracy of the Observations". Shortly after the publication of the book, some gentlemen of Glamorgan invited Lhwyd to undertake the preparation of a natural history of Wales. Nothing suited the impecunious Lhwyd better, provided he could raise sufficient funds to cover his travelling expenses. He therefore caused to be printed a pamphlet outlining an ambitious "Design of a British Dictionary, Historical and Geographical; with an Essay Entituled, Archaeologia Britannica: and a Natural

* "Early Science in Oxford." By R. T. Gunther. Vol. 14: Life and Letters of Edward Lhwyd. Pp. xv+576+14 plates. (Oxford: A. E. Gunther and Dr. Mavis Gunther, c/o University Press, 1945.) 42s.

History of Wales", in which he solicited annual money payments from parties who were interested in the project. His appeal, which was circulated in London and in Wales, attained its object. He therefore prepared a further circular, a questionnaire, in which he sought information from local residents about the geography, antiquities and natural history of the districts he proposed to visit. This also was widely distributed by the somewhat uncertain methods then available, not only in England and Wales, but eventually in Scotland and Ireland as well.

Having thus prepared the ground, early in 1697 Lhwyd set out on his travels. These were to last several years, though latterly the subscriptions promised by his patrons were not all forthcoming. First Wales was revisited, and then he crossed to Ireland, where he was welcomed by members of the Dublin Philosophical Society, and also visited the Giant's Causeway. From Ireland he went on to Scotland, where he met the distinguished Edinburgh naturalist and physician, Sir Robert Sibbald. In a letter written from Bathgate, near Linlithgow, in December 1699, he informed Martin Lister that he had "sped well enough in ye Highlands as to some materials for ye Archaeologia Britannica". Ireland was revisited, and then he returned to Wales. Towards the end of 1700 we find him in Cornwall and, early in 1701, he set out for Brittany. His journeys up to now had not been without minor mishaps, but in France he met with a somewhat more serious misadventure. He was arrested as a spy, and spent eighteen days in confinement in a fortress in Brest. Conditions on the Continent were such that he had to cut short his visit to France, and he returned to Oxford in March 1701.

The remainder of his life was spent in and around Oxford, interrupted only by a short visit to Cambridge to examine certain manuscripts. Apart from his duties as keeper of the Ashmolean, a post that he retained until his death in 1709, his time was spent in digesting and preparing for publication the vast stores of information accumulated during his travels, and sent to him by correspondents. Unfortunately, little of this reached the stage of publication. In 1707, however, volume 1 of "Archaeologia Britannica" was printed in Oxford. It bore the sub-title "Glossography", and was devoted to comparative Celtic philology. Among those competent to appreciate it, it seems to have been looked on as a work of great merit, and Ellis quotes the statement of one authority, that Lhwyd "was in many respects the greatest Celtic philologist the world has ever seen"[1]. Unfortunately, this somewhat abstruse work did not please the Welsh squires who had subscribed towards paying for a natural history of Wales.

Owing to Lhwyd's preoccupation with philological and archæological matters during his last years, his botanical and geological studies seem to have been suspended, though his letters show that he had not entirely lost interest in these subjects. In fact, there is little doubt that it was his intention to publish a natural history of Wales, had he survived, and this, no doubt, would have contained much of interest to botanists and geologists. Unfortunately, after his death, his manuscripts were dispersed, falling into private hands, and, later, by accident, many were burnt.

Thus, apart from the "Lithophylacii", Lhwyd published comparatively little that is of interest to naturalists. He contributed a number of papers to the *Philosophical Transactions of the Royal Society*, including some of botanical and geological interest. His contribution to Camden's "Britannia" included botanical notes; and his friend Ray made use of botanical material supplied to him by Lhwyd. He also supplied Martin Lister with shells that were figured by the latter in his publications on conchology. In passing, it may be mentioned that he contributed material used by Ray in his "Collection of English Words not Commonly Used".

Gunther stresses especially Lhwyd's geological work. Certainly in his earlier years the study of fossils occupied much of his time, and his "Lithophylacii" is a work of very considerable interest and merit. It is remarkable for the accuracy of its illustrations, and for the fact that the fossils figured are carefully localized. Lhwyd is also remembered, among palæontologists especially, as the first to figure a specimen of the extinct order of trilobites. Yet to claim, as Gunther does, that Lhwyd is to be regarded as the father of British palæontology is, perhaps, to allow enthusiasm to outstrip judgment. His views on the origin of fossils were not so sound as those of his contemporary, Robert Hooke, for example. A lengthy exposition of these views is contained in a letter to Ray which has been reproduced by Gunther (No. 200). In this he refers to the work of Steno; but he does not seem to have profited much from the enlightened views of the latter.

The fact that Lhwyd's genius and energies were dissipated in the study of so many branches of learning possibly accounts for his failure to attain lasting fame in any one branch of natural history, and his influence on the thought of his contemporaries may perhaps have been of more importance than his publications. At all events, he occupies a definite niche in the scientific life of the period, no account of which would be complete without something more than a passing mention of his activities. The gap in our knowledge has now been filled by Dr. Gunther, who has collected into one volume most, if not quite all, the information about Lhwyd's work and career that has survived. The sources from which he has drawn this information are scattered and in many cases not easily accessible, and in producing this most readable book there is no question but that he has performed a most useful service, which does full justice to the memory of a neglected naturalist. It should also prove a useful tool for serious students of the history of science. With this in mind, it is as well to point out that one or two sources of information appear to have been overlooked by Gunther. He makes no reference to an interesting though short memoir on Lhwyd published in 1777[3], nor to three letters to Lhwyd, of Scottish interest, that have been published among the early letters of Robert Wodrow[4]. It is clear, though it is not specifically stated, that not all the Lhwyd correspondence known to Dr. Gunther has been reproduced. It may also be mentioned that the Catalogue of Published Works of Edward Lhwyd (Appendix A) is incomplete, and inaccurate in one or two particulars. Attention has been directed to these faults to put the reader on his guard, but they detract little from the merits of this interesting book.

[1] *Trans. Hon. Soc. Cymmrodorion*, 1 (1906–7).
[2] *Trans. Hon. Soc. Cymmrodorion*, 51 (1906–7).
[3] Owen, N., "Memoirs of the Life of Edward Lhwyd, M.A., &c.", in "British Remains" (London, 1777).
[4] *Pub. Scot. Hist. Soc.*, iii, 24, 148, 187 and 263 (Edinburgh, 1937).

OBITUARIES

Mr. A. B. Jackson

MR. ALBERT BRUCE JACKSON, who for many years has been an authority on the identification of hardy trees and shrubs, died on January 14 at his home, in the village of Kew Gardens, Surrey, after a short illness. He was nearly seventy-one years of age, having been born at Newbury on February 14, 1876.

On leaving school at the age of sixteen, Jackson began work as a journalist; but it did not appeal to him and he studied botany in his spare time, paying particular attention to the British flora. Some four or five years later his work on British botany attracted the attention of the director of the Royal Botanic Gardens, Kew, who offered him a post as temporary technical assistant in the Herbarium, where he would have an opportunity of furthering his botanical studies. Soon after, Mr. H. J. Elwes and Dr. A. Henry, when preparing their great work, "The Trees of Great Britain and Ireland", wanted an assistant who could look up botanical references and go about the country collecting information about rare and remarkable trees, and Jackson was given the post. It proved to be a very useful appointment, for not only did it enable him to lay the foundation for his future work upon trees and shrubs, but also, as Elwes and Henry had been given facilities for carrying out their work at Kew, Jackson was able to maintain his connexion.

On the recommendation of Mr. Elwes, Jackson undertook in 1910 to prepare a catalogue of the trees and shrubs in the gardens at Syon House, and that was the forerunner of what became a specialized part of his spare-time work. Later, he catalogued the trees and shrubs at Yattenden Court, Albury Park, Westonbirt, Bordo Hill, and so late as last year, Dawyck.

In 1910, the director of the Royal Botanic Gardens recommended Jackson for an assistant's post in the Imperial Institute, which he retained until 1932; since when, part of his time has been occupied with specialized work in the Department of Botany in the British Museum. In addition to his other activities he found time to coach a number of people in the identification of trees and shrubs, including the Duchess of Northumberland at Albury Park, and the late Duchess of Bedford at Woburn. It was at the suggestion of the Duchess of Bedford that the collection of trees and shrubs at Woburn was placed under his charge, and he visited the estate periodically for many years. During later years he has advised on the care of the trees for the authorities at Stowe School.

Although I had known Jackson from the date of his entrance into Kew, it was not until 1920 that we came into close contact. I had for several years been collecting material for a book on the Coniferæ, and had got a good start, when a mutual friend told me that Jackson had a similar book in view. I therefore suggested collaboration, and after discussion we agreed that he should undertake the descriptions of hardy species and prepare keys to genera and species, I, on my part, undertaking the descriptions of all other species and supplying cultural and economic information. However, he was working hard at the Westonbirt Catalogue at the time, and in the end undertook the four genera, *Abies*, *Cupressus*, *Juniperus* and *Picea* with the whole of the keys. Through his friendship with Miss Lister he was able to arrange with her to make and supply, free of cost, the many life-size drawings of Conifers which are such a distinct feature of the book. He was very keen on making keys to genera and species, and had often been asked to produce a small book giving keys to the Coniferæ with a few details of identification of the kinds commonly grown in gardens. That book was published in 1946. Among his other activities, Jackson was a member of the Scientific Committee of the Royal Horticultural Society.

W. DALLIMORE

Mr. William Barnicot, M.B.E.

MR. WILLIAM BARNICOT, who had been secretary of the Rothamsted Experimental Station since January 1, 1920, died suddenly on December 30. He was born on February 10, 1872, at Shepley, near Huddersfield, and was educated at Marlborough, where he took a prominent part in athletics. Before going to Rothamsted he managed the family business of woollen manufacture, and during the First World War he was commissioned in the R.N.V.R.

Mr. Barnicot's twenty-seven years service as secretary of Rothamsted covered a period of remarkable expansion in agricultural research. The experience of the War of 1914–18 led the Government to devote considerable sums to the development of agricultural science, and both in buildings and in staff Rothamsted shared fully in the increase. Throughout this period of expansion a great deal of work necessarily fell on Mr. Barnicot, which he did with his customary unflurried efficiency. His labours were recognized in 1934 by the award of the M.B.E. In the disturbed conditions of the Second World War, the celebration of the Rothamsted centenary in 1943, although necessarily on a restricted scale, involved much more arduous preparation, in which Mr. Barnicot took a prominent share.

After two brief but serious illnesses in 1941 and 1942, Mr. Barnicot might have retired, but at the special request of the Ministry of Agriculture he agreed to continue until 1947. His loss will be severely felt not only by his colleagues but also by a large number of agricultural scientists throughout the world who have worked at or visited Rothamsted, and with whom he formed many friendships.

WE regret to announce the following deaths:

Sir John Flett, K.B.E., F.R.S., director of the Geological Survey of Great Britain and the Museum of Practical Geology during 1920–35, on January 26, aged seventy-seven years.

Prof. W. L. Jepson, emeritus professor of botany in the University of California, on November 7, aged seventy-nine.

Prof. H. P. Lewis, professor of geology in the University College of Wales, Aberystwyth.

Dr. C. T. Madigan, lecturer in geology in the University of Adelaide and well known for his journeys of exploration in Central Australia, on January 14, aged fifty-seven.

Prof. Karl Mannheim, professor of education in the University of London, on January 9.

Hr. J. W. Sandström, director until 1939 of the Stockholm Meteorological and Hydrographic Institute, aged seventy-two.

NEWS and VIEWS

Genetics at Edinburgh: Prof. C. H. Waddington

Dr. Conrad Hal Waddington, formerly of the Department of Zoology, University of Cambridge, has been appointed to the chair of animal genetics in the University of Edinburgh as from January 1, 1947. This chair has been vacant since the end of 1944 when Prof. F. A. E. Crew, its former and first occupant, was transferred to the chair of public health and social medicine. Simultaneously with this announcement comes the intimation that a headquarters of the National Animal Breeding Research Organisation, a creation of the Agricultural Research Council, is being set up in Edinburgh, and that the Genetics Laboratory of this Organisation, which has been occupying temporary accommodation at Hendon, will be transferred to Edinburgh and accommodated in the Institute of Animal Genetics, where it will work alongside the University staff. Prof. Waddington will retain his position as chief geneticist in this Organisation.

Prof. Waddington is a man of many parts. He took his degree at Cambridge in geology; but soon he was displaying his peculiar aptitude as an experimental zoologist by adapting the standard tissue culture technique to the purposes of a very complete investigation of the formation of the embryo. He was successful in showing that the parts of the developing avian and mammalian embryos determine one another's growth just as they were known to do in amphibians. In joint work he studied the chemical nature of the evocator of neural differentiation and the substances which can replace it. For this work he received from the Royal Academy of Belgium the first award of the Albert Brachet Prize given annually for the best published work on embryology. He became a lecturer in zoology at Cambridge in 1932, holding this appointment until 1945 and taking the degree of Sc.D. in 1938. As an illustration of his versatility, it is to be recorded that after taking his degree he held the Arnold Gerstenberg studentship at Cambridge, which is for the encouragement of philosophy among natural scientists. Prof. Waddington later turned to the study of the normal metamorphosis of *Drosophila melanogaster* and the effects on development of many mutant genes, determining in this way and in considerable detail the part played by normal genes in controlling development. During the war years he served with the Coastal Command R.A.F. as scientific adviser to the Commander-in-Chief.

Prof. Waddington's task at Edinburgh will be twofold. On one hand he will be responsible for the conduct of the research basic to the scientific breeding of farm livestock, that is, for the essential 'objective fundamental' research. On the other, he will lead a University department with all its normal functions; that department will produce geneticists for service either in the National Animal Breeding Research Organisation or elsewhere, and in its research on genetics will be unfettered by any restrictions of application. These changes will mean that the opportunities for genetic research in Edinburgh, both fundamental and applied, will be greatly expanded, and this, taken in conjunction with developments projected under the new Research Organisation in other parts of Great Britain, should ensure the full application of the science of genetics to animal breeding.

Atomic Energy Act: Exemption Order

Section 11 of the Atomic Energy Act limits the disclosure of information in the field of atomic energy, and the wording of this 'secrecy clause' is such as to include many types of scientific apparatus, under the heading of "plant", and many types of scientific experiments as "production of atomic energy". In the Parliamentary debate, the Minister of Supply had announced his intention of using his powers of exemption under the Act to make an Order which would release from the prohibition the tools of the nuclear physicist and, so far as possible, their results. This Order has now been made (Statutory Rules and Orders, 1947, No. 100, H.M. Stationery Office, 1*d*.). Broadly speaking, it exempts from secrecy any apparatus for the investigation of nuclear reactions, except where the reaction is fission of an element of atomic number greater than 89 and results in a positive energy balance. It also exempts detecting apparatus (such as counters), accelerators (such as cyclotrons) and apparatus for generating charged or uncharged particles (including gamma-rays) by means of nuclear reactions, except by means of chain reactions from fission. The wording of the Order refers to the purpose for which the apparatus is to be operated, and presumably if any such apparatus is designed to be used for producing atomic power, or a weapon, disclosure of the details would remain prohibited except by permission of the Minister. This Exemption Order refers, of course, only to the prohibitions of the Atomic Energy Act, and does not affect the position as regards information obtained from official sources, which remains subject to the Official Secrets Act.

Atomic Energy—Destruction or Construction?

An instructive series of exhibits has been brought together by the *Daily Express* at the Dorland Hall, Lower Regent Street, London, to demonstrate some of the facts and potentialities of the controlled release of nuclear energy. The exhibition will remain open during February (weekdays, 10.30 a.m.–7 p.m.; Sundays, 2–7 p.m.; admission, 1*s*.). The first corridor illustrates by models and diagrams the structure of the atom, with particular reference to uranium. Then come models of an alchemist's laboratory and the Cambridge High Tension Laboratory, suggesting the change of technique employed in the sciences, leading to a model of the uranium pile and photographs of American plant built for making fissionable material. A considerable section of the exhibition is devoted to the effects of the first three bombs, centring around a large model of Hiroshima, on which the affected areas are demonstrated, and including photographs and also specimens of material affected by the 'flash'. On the constructive side, a model and diagrams are shown suggesting how atomic piles can be used for the production of power, and some of the uses of artificial radioactive elements are suggested. In the cinema which concludes the tour of the exhibition, selected films are shown of the explosions of the two atomic bombs at Bikini, and portions of a captured Japanese film illustrating the effect of the flash on human beings. It is difficult to say which will produce the more lasting impression: the demonstration of the means of releasing nuclear energy and its peace-time potentialities, or the terrible effects of the atomic bomb. The latter emphasize the desperate need for international co-operation and understanding in this

field. Since most of the material exhibited has come from official sources, one may well ask why the Government itself could not have arranged such an exhibition.

Clay Minerals Group of the Mineralogical Society

THE Mineralogical Society of Great Britain and Ireland has recently approved the formation of a Clay Minerals Group. The study of these minerals not only constitutes a separate branch of mineralogy, but also is of interest to scientific workers in a large variety of departments of pure and applied research. It was felt by the organisers that a Group of this sort would serve an important function in bringing together these workers and allowing them to exchange ideas based on their common interest in clay minerals. For this purpose provision was made for the holding of regular meetings at which accounts could be given of research in progress, and informal discussions could take place. The first meeting of the Group was held on January 24, and invitations to attend it were sent not only to the nucleus of interested persons in the Mineralogical Society itself, but also to a large number of workers in industrial, pure and applied research likely to be interested. The response was most gratifying, and the meeting was held in the rooms of the Geological Society, Burlington House, and was attended by about eighty people. An introductory address was given by Prof. J. D. Bernal.

The following speakers gave accounts of their work: Dr. Mackenzie and Mr. Walker (Macaulay Institute for Soil Research, Aberdeen); Prof. C. H. Edelman (Landbouwhoogeschool, Wageningen); Dr. J. White (University of Sheffield); Mr. K. E. Clare (Road Research Laboratory, Harmondsworth); Prof. Nils Hast (Stockholms Högskola); Dr. Barbara Emödi (Fullers Earth Union, Ltd., Redhill); Drs. A. L. Roberts and G. W. Brindley (University of Leeds); Mlle. Caillère (Museum d'Histoire Naturelle, Paris; S. Hénin (Centre de Recherches Agronomiques, Versailles); Dr. R. K. Schofield (Rothamsted Experimental Station, Harpenden); J. Méring and Mlle. Longuet (Laboratoire Central des Services Chimiques, Paris). In addition, accounts were given of the work in progress at the University of Granada (Spain), and of the work of the Comité Belge pour l'Étude des Argiles. A letter of greeting was received from the latter organisation; also a telegram from Academicians Belyankin and Polynov and other workers at the Dokuchaev Soil Institute, Moscow, which was read at the meeting.

The scientific meetings were followed by a business meeting of the Group members and foreign visitors, at which the functions of the Group, and its relations with similar organisations abroad, were considered. Dr. G. W. Brindley (University of Leeds) was elected president of the Group, and Dr. D. M. C. MacEwan, Rothamsted Experimental Station, Harpenden, Herts, was elected secretary. Promises of support were given by scientific workers from Portugal, Spain and Sweden. M. Hénin gave a promise of collaboration on behalf of the analogous French group, which has recently been constituted (this group differs from the British group, however, in having been set up under Government auspices). Work in Holland in this field was also described. The number of visitors from abroad who were able to attend this one-day meeting was particularly gratifying, and their contributions were very valuable. A reception in honour of the foreign visitors was provided in the evening by the British Council, and they were entertained the following day at Rothamsted Experimental Station.

Halifax House, Oxford

ON January 21, the Chancellor of the University of Oxford, Lord Halifax, formally opened the new Graduate Centre, named in his honour Halifax House, which has successfully been running as a club for senior members of the University since August last. This Graduate Centre was formerly an Anglican convent, standing in two acres of land in Woodstock Road, just north of the Radcliffe Infirmary, and not more than five minutes walk from the main block of science laboratories in the Parks. It is proving a good and timely purchase by the University. The University Grants Committee has helped in financing it. The interior has been skilfully adapted for club use by Mr. J. H. Worthington. Its main purpose is to serve as a club for senior members of the University, especially for those who have, as yet, no college affiliation, for graduates from overseas, for professors from abroad on their sabbatical leaves, for Civil servants from the British Empire back in Oxford for refresher courses and for Colonial and forestry probationers and their like. Halifax House, it is hoped, is but the beginning of a series of clubs in Oxford catering for the needs of senior members which cannot be met by the colleges. It provides, at present, eighteen bed-sitting-rooms for resident members, and two large common rooms and a large garden for non-residents. All meals but breakfast may be had there, and the subscription is only £1 a year.

For several years now the University has had on its conscience the social needs of many of its teachers who tend, at least at first, to be neglected by the colleges. Graduates of other universities appointed to University as opposed to College posts below a professorship, especially in medicine, some sciences and modern languages, may easily find themselves neglected by the colleges and so 'out of things' socially. A large and important modern 'university' now exists in Oxford side by side with the older university with its colleges and great social amenities, and the latter have not done all they could to hold out a friendly, social hand to the former. In the School of Engineering, for example, several of the demonstrators and lecturers have had to wait years before a college noticed their social existence. In the thirty years since the chair was founded, only one teacher below the rank of professor has been made fellow of a college. Halifax House, and similar centres for senior members in Oxford, are bound to play a useful part in bringing together graduates and teachers of many kinds in a social and friendly way who, hitherto, have met little outside working hours.

Production of Oil from Coal in Germany

DR. A. PARKER, director of fuel research, Department of Scientific and Industrial Research, speaking on "Oil from Coal in Germany" at the Fuel Luncheon Club on January 23, stated that Germany developed before the War two synthetic processes, hydrogenation and the Fischer – Tropsch process for the production of petroleum from coal, and many large plants were constructed. At the time of maximum production during the early months of 1944, these two processes together provided oil at a rate of nearly four million tons a year. As a by-product from the

manufacture of gas and coke from coal, another one million tons were obtained. The quantity obtained from home-produced natural petroleum was only about two million tons. Natural petroleum gave most of the lubricating oil used, but hydrogenation processes provided almost the whole of the high-grade aviation spirit. The synthetic processes were much more costly than natural petroleum, but with Germany's policy of self-sufficiency cost was not of first importance. It has been estimated that with present-day prices of labour, coal and other materials, the cost of obtaining oil from coal in Great Britain by the processes as developed and operated in Germany would be at least two shillings a gallon, as compared with only about sixpence for imported petroleum, excluding import duties. Research should be continued, however, as there are prospects of improving the methods of obtaining oil from coal, and in certain parts of the British Commonwealth there are cheaper supplies of coal than in Great Britain, as well as reserves of brown coal and lignite. Dr. Parker stated that bombing attacks between May and September 1944 caused a reduction in German production of synthetic oil from a rate of nearly four million tons a year to only about 300,000 tons. During the following months, with a reduction in bombing and determined efforts in Germany to repair damage, production again rose to a rate of one million tons a year in November 1944, after which it again fell to 150,000 tons a year at the end of February 1945. It is certain that this systematic destruction of German oil plants was one of the main factors in hastening the defeat of Germany.

Proposed International Exhibition in 1951

THE proposal that an International Exhibition should be held in London in 1951, with other matters relating to exhibitions, was referred by the Secretary for Overseas Trade to a representative Committee (the Ramsden Committee), which, in December 1945, recommended that an international exhibition should be held in London in 1951 or as soon as possible afterwards, and that it should be sited in Central London. The Government accepted the suggestion, but there have been difficulties in finding a site, accommodating visitors, etc. The Royal Society of Arts, which as the Society of Arts took the initiative in promoting the International Exhibition held in Hyde Park in 1851, has joined in advocating the holding of another and far greater Exhibition in 1951 to mark the centenary. The Society has accordingly called a conference for February 6, to give fuller consideration to this question and to discuss in particular: (1) whether an International Exhibition on a large scale should be held in London in the near future; (2) if so, whether Hyde Park should be the site; (3) whether every effort should be made to hold it in 1951, the centenary year of the first International Exhibition. Should the conference approve the general proposal, a formal approach would then be made to the London local authorities and to the Government.

Journal of Research of the National Bureau of Standards

WITH the July issue, which began the thirty-seventh semi-annual volume, the *Journal of Research of the National Bureau of Standards*, a monthly periodical published by the U.S. Department of Commerce and devoted to pure and applied science, appeared in a new typographical style and size. A larger size of page now brings the *Journal* into line with many other American scientific and technical magazines, such as the publications of the American Physical Society, and with that recently adopted for the Bureau's *Technical News Bulletin*. Other improvements in the general format are a light blue cover, a separate 'contents' page, double reading columns, two-point spaces between the lines of text and tabular matter, and modern type for display heads. These changes make the printed page much easier to read. As in former issues, the individual articles are still printed as separate units, and reprints are made available for purchase as 'Research Papers' from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C., shortly after their appearance in the monthly issue of the *Journal*. In this respect it may be of interest to readers to know that H.M. Stationery Office has recently announced that arrangements have been made for that Office to act as agents for U.S. Government publications, and that orders should be sent to the London Sales Office, York House, Kingsway, W.C.2.

The Research Papers, Nos. 1723–29 inclusive, which appear in the July issue, deal with radio-proximity-fuse design for bombs, rockets and mortars which were mainly developed during the Second World War; laboratory distilling columns for the analysis and purification of hydrocarbons; nickel plating on steel by chemical reduction; the temperature coefficients of proving rings used in the calibration of test machines applying forces to engineering materials and structures; the mutarotation and ring structure of mannuronic lactone; the heats of formation and combustion of certain CH_2 groups; and a description and analysis of the spectrum of uranium between 2900 and 11000 A.

Intrinsic Safety of Electrical Apparatus

THE use of electrical appliances in coal mines needs to be safeguarded against the risk that they may be the means of igniting firedamp (or coal dust) and thus starting an explosion. Two methods are commonly employed. One is to provide a suitable enclosure from which flame cannot be ejected. The other method is to make the apparatus and related electrical circuits 'intrinsically safe', which means that any sparking which may occur when the circuit is broken at any point is incapable of causing ignition. One aspect of the latter method is discussed in a report, Paper No. 104, published recently by the Safety in Mines Research Board. It gives the relationship which has been found between the inductance in a circuit when the circuit is rapidly broken between a pointed platinum electrode and a wire and the minimum current capable of igniting firedamp. The effects of two methods of increasing the minimum igniting current have been measured. These are the introduction into the circuit of a shunting resistance or condenser. They can be of considerable value when the inductance is high and the current correspondingly low, as in apparatus such as bells, telephones, signalling and shot-firing systems, but become almost valueless when the working current exceeds about one ampere, or in the case of condenser shunts when a slow break of the circuit can occur. The results reported in this paper will be used at the Safety in Mines Testing Station in the examination of mining electrical apparatus submitted by the makers for approval.

"Faraday's Encyclopædia of Hydrocarbon Compounds"

THIS work and the first replacement-addition for Vol. 1 have already been reviewed in *Nature* (April 21, 1945, p. 467; Dec. 22, 1945, p. 744); the second replacement-addition (C_1–C_4) and one for (C_5 and C_7) are now to hand (Manchester: Chemindex, Ltd. 25*s*. and 19*s*. 6*d*. respectively). These follow much the same lines as the original and complete the subject-matter of the Encyclopædia to January 1, 1946. Certain definite improvements making for easier reference have been introduced, as, for example, the recasting of the list of journals so that abbreviations may be more easily traced; more comprehensive treatment of physical properties and the marking of methods of preparation which may be of value in the laboratory will also increase the utility of the work. As before, the labour of changing the sheets and adding the new ones is simple. The author is to be heartily congratulated in being so close to the 'literature front'. Volume C_5 is being printed, and volumes C_6 and C_{10} are promised shortly; new replacement-additions will not be issued until 1948, when it is hoped that all existing volumes will be completed to January 1, 1948.

University of London Appointments

The following appointments have been announced:

Dr. J. J. C. Buckley, to the William Julien Courtauld chair of helminthology tenable at the London School of Hygiene and Tropical Medicine, as from October 1, 1946. He was appointed demonstrator in helminthology at the School in 1927. In 1931 he was awarded a Wandsworth Scholarship and was engaged on research work abroad until 1938, when he became a lecturer at the School. In 1940 he was sent to investigate human onchocerciasis in Kenya, and in 1944 he carried out a helminthological survey in Northern Rhodesia.

Dr. D. W. Smithers, to the University chair of radiotherapy tenable at the Royal Cancer Hospital as from October 1, 1946.

Mr. G. A. Barnard, to the University readership in mathematics tenable at the Imperial College of Science and Technology as from October 1, 1946. During 1940–42 he worked as a research physicist with the Plessey Co., Ilford, and afterwards he went to the Department of the Controller of Physical Research and Signals Development of the Ministry of Supply. Since 1944 he has been a lecturer in the Department of Mathematics at the Imperial College.

The title of professor emeritus of morbid anatomy in the University has been conferred on Prof. H. M. Turnbull, who held the chair of morbid anatomy at the London Hospital Medical College from February 1919 until his retirement in September 1946. The title of reader in anatomy in the University has been conferred on Dr. R. W. Haines, in respect of the post held by him at St. Thomas's Hospital Medical School. The title of reader in palæontology in the University has been conferred on Dr. Alan Wood, in respect of the post held by him at the Imperial College of Science and Technology.

Colonial Office Appointments

THE following appointments in the Colonial Services have recently been made: G. S. Keeping, to be agricultural officer, Malaya; P. H. Nye, to be specialist agricultural chemist, Gold Coast; D. A. Harkin, to be geologist, Tanganyika; Captain P. C. Bindon, to be surveyor, Uganda; D. G. Glassford, to be surveyor, Sierra Leone; G. Corbett (tobacco officer, Mauritius), to be superintendent of agriculture, Cyprus; G. C. Stevenson (geneticist, Department of Agriculture, Mauritius), to be sugar agronomist and plant breeder, British Guiana.

The Night Sky in February

FULL moon occurs on February 5d. 15h. 50m. U.T. and new moon on February 21d. 02h. 00m. The following conjunctions with the moon take place: Feb. 4d. 20h., Saturn 4° S.; Feb. 13d. 04h., Jupiter 0·02° S.; Feb. 16d. 23h., Venus 5° N.; Feb. 20d. 02h., Mars 4° N.; Feb. 22d. 09h., Mercury 7° N. Mercury rises at 8h. 15m., 8h., and 7h. at the beginning, middle and end of the month, respectively, and attains its greatest easterly elongation on Feb. 21. The stellar magnitude of the planet varies between −1·1 and −0·4 during the month. Venus is conspicuous as a morning star, rising at 4h. 43m., 4h. 56m., and 5h. 02m. at the beginning, middle and end of the month, respectively, the stellar magnitude varying between −4 and −3·8 during February. Mars is too close to the sun to be observed. Jupiter, in the constellation of Libra, rises at 2h. 26m., 1h. 41m. and 0h. 52m. at the beginning, middle and end of the month, respectively, its stellar magnitude being about −1·6. Saturn, in the constellation of Cancer, is visible throughout the greater portion of the night, setting a little before the sun at the beginning of the month. The stellar magnitude of Saturn is about 0·2 in February. Only one occultation of stars brighter than magnitude 6 takes place in February: Feb. 1d. 00h. 08·3m., 227 B.Taur. (*D*). The latitude of Greenwich is assumed, and *D* refers to disappearance.

Announcements

PRINCESS ELIZABETH has been elected an honorary member of the Institution of Civil Engineers. Her Royal Highness thus becomes the first lady honorary member. Ladies have been considered eligible for election to corporate membership under the charters in view of the provisions of the Sex Disqualification (Removal) Act, 1919, and the first lady member was elected in 1927.

PROF. T. H. HAVELOCK, lately professor of mathematics at King's College, Newcastle-on-Tyne, in the University of Durham, and W. D. Lambert, chief of the Section of Gravity and Astronomy of the U.S. Coast and Geodetic Survey, have been elected *Correspondants* for the Section of Geography and Navigation of the Paris Academy of Sciences.

PROF. A. V. HILL, Foulerton research professor of the Royal Society, Sir Jasper Ridley, a director of Coutts and Co. and of the National Provincial Bank, and Mr. G. M. Young, a member of the Standing Commission on Museums and Galleries, have been elected trustees of the British Museum.

THE Tilden Lecture of the Chemical Society will be delivered by Dr. A. E. Alexander on "The Application of Surface Chemistry to Colloid Problems" on February 6 at 7.30 p.m.

IN connexion with the announcement of the award of the Charles Vernon Boys Prize of the Physical Society in *Nature* of January 4, p. 21, we have been asked to point out that the word 'Skiatron' is a trade mark registered in the name of Scophony, Ltd., to describe dark trace tubes.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Discovery of Molecular Asymmetry

PASTEUR'S two lectures on molecular asymmetry (Paris, 1860 [1]) are justly regarded as a supreme example of the characteristic lucidity of the French mind and language. Organic chemists point with pride to the fact that scarcely a word of this classic needs to be altered at the present day.

There is, however, one slight error contained in a speculation over-generalizing the phenomenon which we now call internal compensation, and which Pasteur was the first to encounter in the case of mesotartaric acid. To be more precise, it seems that Pasteur related *dl*-malic acid to mesotartaric acid rather than to racemic acid. This misconception was cleared up by the work of Le Bel and van t'Hoff in 1876 and is of little historical importance. Pasteur also spoke of twisted and spiral molecules and of untwisting them (racemization), but only, in his own words, as a "rough metaphor".

These passages are used by Prof. G. F. Gause[2] in justification of the remarkable pronouncement that "Pasteur did not distinguish sharply the disymmetry of individual molecules from the asymmetry of their aggregates in the sense given above". As the excellent monograph, dated 1941 but only just available in Britain, in which this surprising statement occurs, will be widely read, it is perhaps worth while to place on record an individual opinion that Pasteur did indeed make this distinction, and that he made it in the exact sense in which it is understood at the present time. As the point is so well appreciated by the great majority of chemists and physicists, it would be useless to labour it. Let any doubter read the "Lectures on Molecular Asymmetry" for himself. These lectures are built on the foundation of the conceptions of molecular asymmetry and of the mirror-image forms of molecules, and a clear contrast with such a case as that of a quartz crystal is emphasized. Two citations may be given from the English translation.

"We know, on the one hand, that the molecular structures of the two tartaric acids are asymmetric, and on the other, that they are rigorously the same, with the sole difference of showing asymmetry in opposite senses. Are the atoms of the right acid grouped on the spirals of a dextrogyrate helix, or placed at the summits of an irregular tetrahedron, or disposed according to some particular asymmetric grouping or other? We cannot answer these questions. But it cannot be a subject of doubt that there exists an arrangement of the atoms in an asymmetric order, having a non-superposable image."

"Quartz! you will say at once. We saw in the last Lecture that quartz possesses the two characteristics of asymmetry—hemihedry in form, observed by Haüy, and the rotative phenomenon discovered by Arago! Nevertheless, molecular asymmetry is entirely absent in quartz. To understand this, let us take a further step in the knowledge of the phenomena with which we are dealing. We shall find in it, besides, the explanation of the analogies and differences already pointed out between quartz and natural organic products.

"Permit me to illustrate roughly, although with essential accuracy, the structure of quartz and of the natural organic products. Imagine a spiral stair whose steps are cubes, or any other objects with superposable images. Destroy the stair and the asymmetry will have vanished. The asymmetry of the stair was simply the result of the mode of arrangement of the component steps. Such is quartz. The crystal of quartz is the stair complete. It is hemihedral. It acts on polarized light in virtue of this. But let the crystal be dissolved, fused, or have its physical structure destroyed in any way whatever; its asymmetry is suppressed and with it all action on polarised light, as it would be, for example, with a solution of alum, a liquid formed of molecules of cubic structure distributed without order.

"Imagine, on the other hand, the same spiral stair to be constructed with irregular tetrahedra for steps. Destroy the stair and the asymmetry will still exist, since it is a question of a collection of tetrahedra. They may occupy any positions whatsoever, yet each of them will none the less have an asymmetry of its own. Such are the organic substances in which all the molecules have an asymmetry of their own, betraying itself in the form of the crystal. When the crystal is destroyed by solution, there results a liquid active towards polarised light, because it is formed of molecules, without arrangement, it is true, but each having an asymmetry in the same sense, if not of the same intensity in all directions."

Prof. Gause has identified one of Pasteur's metaphors with the theory that they illustrated.

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Jan. 6.

[1] "Leçons de Chimie professées en 1860" (Chemical Society of Paris, 1861, Alembic Club Reprint No. 14).
[2] "Optical Activity and Living Matter", *Biodynamica* (1941), cf. p. 10, footnote 3.

Mixed Crystal Formation in High Polymers

AN interpolymer of ethylene and vinyl alcohol (prepared by co-polymerization of vinyl acetate and ethylene followed by hydrolysis[1]) has been found to give a crystalline X-ray diffraction pattern different from those of either polythene or polyvinyl alcohol. The fibre repeat distance is 2·5 A., corresponding to a single hydrocarbon chain zigzag. This fact alone indicates that there is no regular succession of the different monomer units along the chain molecules, because such a regular arrangement would have a fibre repeat period of at least twice the observed value. The succession of monomer units is thus apparently random, so that this is an example of the crystallization of high-polymer molecules of geometrically irregular structure. Such a crystal can be appropriately regarded as a mixed crystal. We have here yet another analogy between the crystallography of polymeric substances and that of monomeric chemical compounds.

We have studied this and several other examples of isomorphous replacement in crystals of long-chain molecules; this work will be described and discussed in detail elsewhere.

It is of particular interest to try to discover what differences of size and shape can be tolerated in the replaceable units. Thus a single hydrogen atom on

a carbon chain may be replaced by a fluorine or a hydroxyl group, but apparently not by a chlorine or a methyl group; a pair of hydrogen atoms may be replaced by an oxygen atom. It is difficult to discuss the degree of tolerance in quantitative terms, but it would appear that the limits are much wider than for ionic or metallic crystals.

These facts appear to have certain implications. Since a hydroxyl group may replace a hydrogen without destroying the crystallinity in the interpolymer already mentioned, it follows that in polyvinyl alcohol itself the hydroxyl groups might be randomly placed in the alternative space positions on either side of the plane of the carbon chain. It has formerly been assumed that the fibre repeat-distance of 2·5 A. proves that the hydroxyl groups in polyvinyl alcohol are all on the same side of the plane of the carbon chain, in any event within the length of molecule in any one crystallite; and a complete crystal structure based on molecules having this regular configuration has been put forward[2]. The assumption now seems to be not necessarily correct. It has been urged by one of us[3] (for other reasons) that the crystal structure of polyvinyl alcohol should be reconsidered, and we now suggest that alternative structures based on molecules having hydroxyl groups indiscriminately in left- and right-hand positions should be considered.

The second implication affects the theory of crystallinity of chemically homogeneous polymers. We have long considered that those polymers which do not crystallize (for example, polyvinyl acetate and polystyrene) fail to do so because the substituent groups occur indiscriminately in left- and right-hand positions, so that the molecules, though chemically regular, are geometrically irregular; whereas other polymers such as polythene, polyvinylidene chloride and many polyamides and polyesters are crystalline because they have either no substituents or two identical substituents on any one chain carbon atom; these molecules, therefore, are geometrically as well as chemically regular. At the Chemical Society discussion on high polymers held last April, it was pointed out that the properties of polyvinyl alcohol and polyvinyl acetate are difficult to reconcile with this theory. Polyvinyl alcohol and polyvinyl acetate are chemically interconvertible and, therefore, might be supposed to have their substituent groups in precisely corresponding positions; yet polyvinyl alcohol crystallizes well, whereas polyvinyl acetate has never been crystallized. The regularity of polyvinyl alcohol has hitherto been unquestioned, and, on the assumption that polyvinyl acetate must likewise be regular, other explanations of non-crystallization of the latter have been sought. Our present evidence suggests that the argument should be reversed. Thus, we postulate that the acetate, because it is non-crystalline, is irregular; if we assume that the alcohol is likewise irregular, the fact that it crystallizes, which would formerly have seemed a difficulty, is to be attributed to the comparative smallness of the hydroxyl group, which can replace hydrogen in a polymer crystal lattice. Thus, we submit that the above-mentioned theory is essentially sound in correlating crystallinity with geometrical regularity of molecules and non-crystallinity with geometrical irregularity; but that when substituent groups are similar in shape and size, molecules which are chemically irregular may be sufficiently nearly geometrically regular to crystallize.

The third implication is that, even in crystals of chemically regular polymers the molecules of which *can* be geometrically regular, different configurations of monomer units obtainable by rotation around single bonds might occur, provided that the energy contents and the effective external shapes of the alternative configurations are similar. This might possibly occur in rubber; a regular structure which accounts well for the X-ray diffraction pattern has been proposed[4]; but there are two types of configuration, both having about the right fibre repeat-distance, readily interconvertible by rotation around single bonds, and not very different in effective overall shape, and it is not impossible that the two types might be present in the same crystal. Whether a structure comprising comparable proportions of two different molecular configurations is compatible with the X-ray evidence is a matter which can be decided by calculations of the relative intensities of the X-ray reflexions.

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Nov. 21.

[1] Perrin, M. W., Fawcett, E. W., Paton, J. G., Williams, E. G., and I.C.I., Ltd., B.P. 497,643 Ex. 14; and du Pont de Nemours, Inc.; B.P. 582,093.
[2] Mooney, *J. Amer. Chem. Soc.*, **63**, 2328 (1941).
[3] Chapter by Bunn in "Advances in Colloid Science", **2**, 121 (Interscience Publishers, 1946).
[4] Bunn, *Proc. Roy. Soc.*, A, **180**, 40 (1942).

Rate of Crystal Growth in Glass

The shape of the curve relating the rate of crystal growth to temperature of undercooling is familiar as a hump-backed curve. The shape of the low-temperature portion is usually ascribed to viscosity, and that at higher temperatures either to surface tension barriers hindering nucleus formation, or to a rather arbitrary undercooling factor based on the geometry of the phase diagram[1]. The former is inadmissible where the growth of crystals already in existence is being considered, and the latter gives a very poor agreement with the experimental data, due, no doubt, to the arbitrary nature of the assumptions involved.

Examination of the problem in the following manner leads directly to a curve of the right type, though it has proved difficult to substantiate the portion immediately below the liquidus in the example studied.

If a plane glass-crystal boundary is considered, the ions in the glass require a vibrational energy E_1 to enable them to become mobile, while those in the crystal require E_2. Of the glassy ions at the crystal boundary, those which are mobile and have energy less than E_2 will condense, whereas those in the crystal with greater energy will melt, so that the rate of crystal growth (R) will be proportional to

$$\frac{1}{kT}\int_{E_1}^{E_2} \exp - E/kT \,.\, dE - \frac{r}{kT}\int_{E_2}^{\infty} \exp - E/kT \,.\, dE, \quad (1)$$

where r is the ratio of the surface densities of crystal and glass.

Thus,

$$R = A \exp - E_1/kT \left\{1 - (r+1) \exp \frac{E_1 - E_2}{kT}\right\}. \quad (2)$$

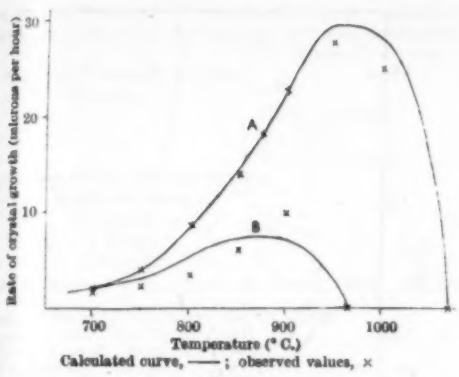

Fig. 1

The curve *A* of Fig. 1 shows the measured and (assuming $r = 1$) calculated crystal growth-rates for cristobalite separating from Pyrex brand glass with liquidus at 1,064° C.

In Fig. 2, log viscosity is plotted against the reciprocal of the absolute temperature (*C*), together with the logarithm of the rate of devitrification (*D*) for points on the left-hand slope of curve *A* (Fig. 1). It is at once seen that the power of this latter exponential (given by the slope of the curve in Fig. 2) is only about a quarter of the value of the corresponding constant for viscosity.

Presumably in the present case the major requirement for crystal growth is the removal of the boron and sodium ions from the Si-O lattice, thereby giving the necessary freedom to collapse to crystal form without any major positional change. The preponderance of cristobalite, considered unstable at this temperature, is then readily explained.

While no information is available for the boron ions, the temperature variation of the mobility of the sodium ions may be inferred from the electrical conductivity measurements. Values for the latter extrapolated from lower temperatures are also shown in Fig. 2 (curve *E*). When the composition of the glass (approximately 80 per cent SiO_2; 12 per cent B_2O_3; 4·6 per cent NaO) is borne in mind, it will be seen that the slope of the low-temperature portion of the curve is in reasonable accord with the above views.

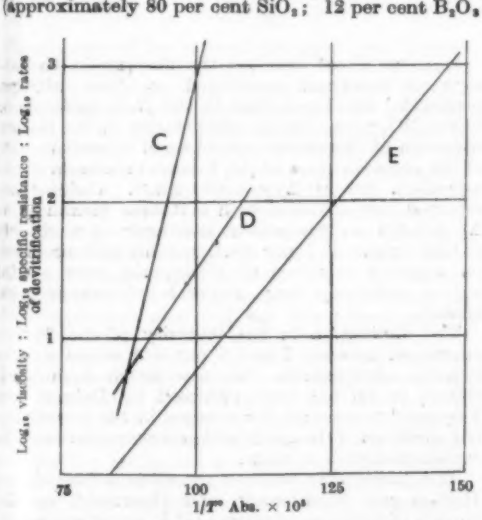

Fig. 2

For other glasses of the same class precipitating cristobalite, the constants A and E_1 in the above formula may be considered to remain substantially unchanged; so that, using their determined values and the liquidus temperature for the new glass, one can anticipate the form of the variation of the devitrification-rate with temperature. Thus the curve *B* (Fig. 1) was determined for a glass containing 21·5 per cent B_2O_3, and the crosses are the observed rates.

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Dec. 13.

[1] Preston, E., *J. Soc. Glass Tech.*, **24**, No. 104 (Aug. 1940).

Ultra-violet Absorption Spectrum of Anhydrous Nitric Acid

Recently, Ingold and co-workers have presented evidence of the existence of the nitronium ion (NO_2^+) in solutions of nitric acid in concentrated sulphuric acid[1,2]. We wish to report some observations on the ultra-violet absorption spectrum and electrical conductance of the system water/nitric acid/nitrogen pentoxide which are of relevant interest.

The absorption maximum of the nitrate ion at 3020 A., present in the spectrum of dilute aqueous solutions of nitric acid, diminishes in intensity as the acid concentration is increased. Above about 60 per cent acid concentration, this maximum is not seen, and there appears instead a maximum near 2600 A.[3,4,5,6]. A maximum at this position was noted also by von Halban and Eisenbrand[3] in the spectrum of a solution of nitric acid in anhydrous ether, and attributed by these authors to the undissociated HO–NO_2 molecule. Hantzsch[6] failed to confirm this, and reported that in anhydrous ether nitric acid shows no absorption maximum.

Our measurements have extended over a concentration range from 80 per cent nitric acid – 20 per cent water to 76 per cent nitric acid – 24 per cent nitrogen pentoxide. With 20 per cent of water present there is a well-defined maximum at 2650 A. ($E_{max.}$ 9·9, see Curve *A* of Fig. 1; molecular extinction coefficients are calculated for HNO_3). As the water content is decreased, this band becomes stronger and reaches a maximum intensity of 11·7 with 7 per cent of water present (Curve *B*, Fig. 1). With further diminution in the water content the intensity of the maximum falls to a lowest value for 5 per cent water ($E_{max.}$ 9·5, Curve *C* of Fig. 1). With approach to the anhydrous acid the extinction at the maximum rises again, but this second increase is accompanied by considerable flattening of the curve. The spectrum of the anhydrous acid is shown in Curve *D* of Fig. 1.

Comparison with the curves for solutions of nitrogen pentoxide in anhydrous nitric acid show no discontinuity in the neighbourhood of the anhydrous acid. Solutions of nitrogen pentoxide in carbon tetrachloride or acetic anhydride exhibit very intense steeply rising absorption at wave-lengths less than 3000 A.

These data indicate that in aqueous solutions of nitric acid containing between 5 and 20 per cent of water the spectroscopically predominating species is

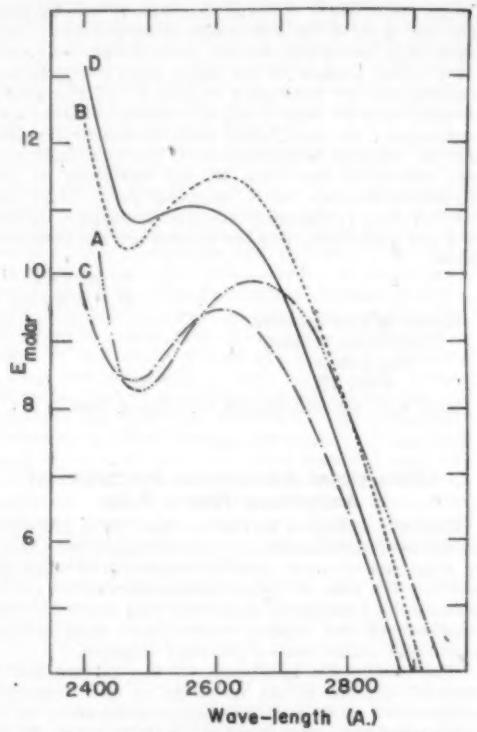


Fig. 1. ULTRA-VIOLET ABSORPTION SPECTRA OF AQUEOUS NITRIC ACID SOLUTIONS. *A*, 20 PER CENT WATER; *B*, 7 PER CENT WATER; *C*, 5 PER CENT WATER; *D*, ANHYDROUS ACID

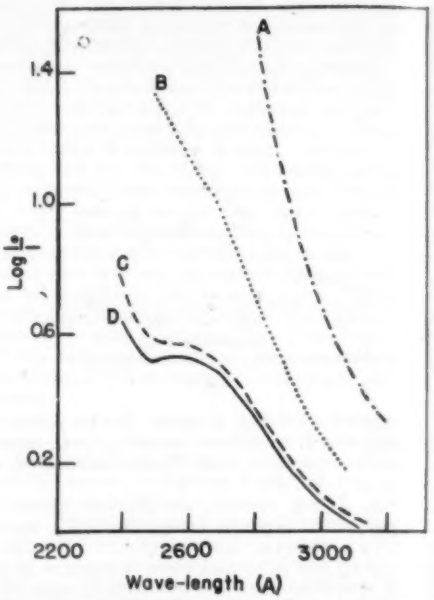


Fig. 2. ULTRA-VIOLET ABSORPTION SPECTRA OF SOLUTIONS OF NITROGEN PENTOXIDE IN ANHYDROUS NITRIC ACID. *A*, 24 PER CENT NITROGEN PENTOXIDE; *B*, 12 PER CENT NITROGEN PENTOXIDE; *C*, 3 PER CENT NITROGEN PENTOXIDE; *D*, ANHYDROUS ACID. (THE OPTICAL DENSITIES HAVE BEEN CORRECTED FOR CHANGE IN THE SPECIFIC GRAVITY OF THE SOLUTIONS)

a molecule or ion designated the '2600A chromogen' which may be the HO–NO_2 molecule. It is in maximal concentration in the presence of 7 per cent of water. When the water content is reduced to less than 5 per cent the spectrum flattens in a manner consistent with the establishment of an equilibrium such as (1) with 'free nitrogen pentoxide' in solution.

$$2\,HNO_3 \rightleftharpoons H_2O + N_2O_5 \quad . \quad . \quad (1)$$

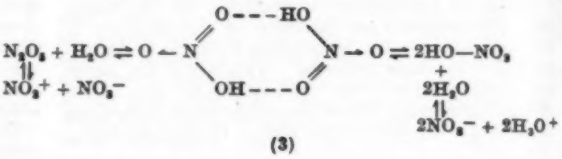
This hypothesis is in accord with the lack of discontinuity noted in passing from the 95 per cent acid to solutions containing excess nitrogen pentoxide (Figs. 1 and 2).

These spectrographic measurements provide no clue as to the condition of the dissolved nitrogen pentoxide, but parallel studies of the electrical conductance of these systems at 25° C. have confirmed the earlier observation of Veley and Manley[7] that the equivalent conductance is minimal for a solution containing about 3 per cent of water and increases as the anhydrous condition is approached. With addition of nitrogen pentoxide to the anhydrous acid the conductance continues to increase, and there is no discontinuity apparent in the neighbourhood of the anhydrous acid.

The sharp increase in conductance with less than 2 per cent water-content is suggestive of the formation of a highly ionized compound and is consistent with the establishment of an equilibrium such as (2).

$$N_2O_5 \rightleftharpoons NO_2^+ + NO_3^- \quad . \quad . \quad (2)$$

Both the spectrographic and the conductance measurements over the complete concentration range are consistent with a system of equilibria as shown in (3).



(3)

It is uncertain whether the changes in the absorption spectrum attributed to 'free nitrogen pentoxide' are due in fact to the N_2O_5 molecule or to the NO_2^+ ion, and an examination of the Raman spectrum of this system would be of interest in view of the identification[3] of the Raman maximum of the nitronium ion at $\Delta\nu = 1{,}400$ cm.$^{-1}$. Chédin[8] has reported that material with a Raman maximum at $\Delta\nu = 1{,}400$ cm.$^{-1}$ is present in anhydrous nitric acid to the extent of 1 per cent, but his measurements on aqueous solutions of nitric acid were carried out at relatively large intervals of concentration change.

The diminution in the intensity of the 2600 A. maximum between 7 and 5 per cent water content remains unexplained. An association such as is shown in (3) has been proposed by Dalmon and Freyman[9] to account for changes in the near infra-red spectrum of the nitric acid–water system over the same concentration range.

The discrepancy between the observations of von Halban and Eisenbrand[4], and Hantzsch[6], on the spectra of solutions of nitric acid in anhydrous ether,

can be understood if Hantzsch's acid contained appreciable amounts of dissolved nitrogen pentoxide.

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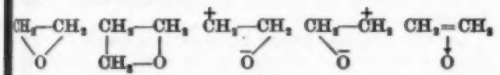
Nov. 29.

[1] Gillespie, Graham, Hughes, Ingold and Peeling, *Nature*, **158**, 480 (1946).
[2] Ingold, Millen and Poole, *Nature*, **158**, 481 (1946).
[3] Hartley, *J. Chem. Soc.*, **83**, 221 (1903).
[4] Schaefer, *Z. anorg. Chem.*, **98**, 77 (1916).
[5] von Halban and Eisenbrand, *Z. phys. Chem.*, **132**, 433 (1928).
[6] Hantzsch, *Z. phys. Chem.*, A, **147**, 161 (1930).
[7] Veley and Manley, *J. Chem. Soc.*, **33**, 1015 (1903); see also Berl and Saenger, *Monatsh. für Chemie*, **53-54**, 1036 (1929).
[8] Chédin, *Ann. Chim.*, **8**, 243 (1937).
[9] Dalmon and Freyman, *C.R. Acad. Sci. Paris*, **211**, 472 (1940).

Structures of Ethylene Oxide and Cyclopropane

The formula of ethylene oxide is commonly portrayed as (I), the instability of the compound being ascribed to the existence of strain tending to open the ring. This explanation can scarcely be accepted, for, if it were true, the compound (II), with less angular strain, should be more stable—which it is not. Zimakov[1] has recently directed attention to this point and to the discrepancy between the structure (I) and the observed properties of ethylene oxide. He points out that the latter (for example, polymerization, addition reactions) strongly suggest a resemblance of structure to ethylene, and proposes to replace (I) by a resonance hybrid of (I), (III) and (IV).



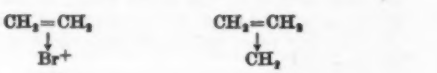
(I) (II) (III) (IV) (V)

The concept of the 'co-ordinate link' type of bond is well accepted in chemistry, and is indeed necessitated by the existence of such compounds as $BF_3.NH_3$. It has hitherto been supposed that the 'donated' electrons must be initially non-bonding, but in molecular orbital theory there is nothing to warrant this limitation. In a recent paper[2] we have generalized the concept to include also donation by bonding electrons and have given many illustrations of problems that then receive a simple explanation. Dewar[3] has independently put forward similar ideas, and must be regarded as the first to develop the new theory. It is true that donation by lone pair electrons is more commonly observed, but this is because such electrons are usually more weakly bound than bonding electrons. It is the study of ionization potentials that provides the knowledge that enables one to say which electrons in a molecule are most likely to be donated. For donation by bonding electrons to occur, it is essential that these electrons should have especially low ionization potentials. π-Electrons in olefines fulfil this condition, though there appears to be no need strictly to limit such donation to π-electrons.

The π-electrons of ethylene lie in an orbital of ionization potential 10·45 V.[4]: the lone pair electrons of ammonia lie in an orbital of ionization potential 10·8 V.[4]. We therefore expect donation of the π-electrons of ethylene, just as donation of the lone pair electrons of ammonia occurs. Just as NH_3 forms NH_4^+, $(C{=}C) \rightarrow H^+$ is expected and plays an important part in acid-catalysed olefine polymerization[2]. $BF_3.C_2H_4$ is expected, just as $BF_3.NH_3$ is known. The formula of ethylene oxide may be portrayed as (V), and an analogy may be drawn between it and the amine oxides. (V) is closely analogous to the complex (VI) that probably plays an important part in the bromination of olefines[2]. The formula means that the two electrons of the original π-bond in ethylene now bind the three nuclei C, C and O. This need cause no surprise when the modern knowledge of conjugated molecules and non-localized bonds is recalled. Each carbon atom is in the trigonal state, forming three planar, hybridized sp^2, σ-bonds at 120°. This is in contrast to (I), where $\angle$ HCH should be close to 109°. The fourth valency of each carbon atom is a pure $2p_z$ orbital of axis at 90° to the plane of the molecule. These $2p_z$ orbitals of the carbon atoms overlap with each other and with the $2p_z$ atomic orbital of an oxygen atom lying alongside with the correct orientation. We therefore construct a non-localized molecular orbital, described in the LCAO approximation as

$$a\psi_A(C : 2p_z) + a\psi_B(C : 2p_z) + b\psi(O : 2p_z).$$

The trigonal configuration of the two carbon atoms and the existence of π-electrons account for the striking parallel between the properties of ethylene oxide and those of ethylene. The formula (V) is simpler than the cumbrous resonance description in terms of (I), (III) and (IV), though it may amount to much the same thing in ultimate thought content.



(VI) (VII)

We have already stressed[2] the importance of the idea of donation of π-electrons to the understanding of the reactions of the methylene molecule. We regard cyclopropane, like ethylene oxide, as having a perturbed ethylene structure (VII). Two very strong pieces of evidence for this view are that (1) substances in which a cyclopropane group is adjacent to a double bond exhibit just those spectroscopic properties which are characteristic of conjugated compounds; (2) the dipole moment of cyclopropyl chloride is 0·3 *D* lower than the value for isopropyl chloride[5]: this is easily understood on analogy with phenyl chloride, but very difficult to understand[5] on the conventional formula for cyclopropane.

Further discussion of these points will be given elsewhere.

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Dec. 7.

[1] Zimakov, *Acta Phys. Chim. U.S.S.R.*, **21**, 401 (1946).
[2] Walsh, *J. Chem. Soc.*, in the press.
[3] Dewar, *J. Chem. Soc.*, 406, 777 (1946).
[4] Sugden, Walsh and Price, *Nature*, **148**, 372 (1941).
[5] Rogers and Roberts, *J. Amer. Chem. Soc.*, **68**, 843 (1946).

Reactions of Organic Halides in Solution

We are sorry to have again to contradict Dr. Evans, but he continues to misrepresent us[1]. He does so now by quoting a limited passage from a paper by one of us[2], and then adding, as though it were a paraphrase, a statement of his own that does not, in fact, reproduce the meaning. The passage is dealing with the relative rates of nucleophilic halogen-substitution in *R*-Hal, where *R* = Me, Et, *n*-Pr, *i*-Bu, *neo*Pentyl, and, as quoted, reads: "The rate relationships for the first four members are fairly normal for the bimolecular mechanism, the rate of which is decreased by electron-accession to the reaction centre, but the introduction of the last β-methyl substituent has obviously introduced an effect which is far larger than that to be expected on the basis of its capacity for the release of electrons. . . . This effect is believed to be of steric origin." Dr. Evans continues: "This indicates that steric hindrance was considered to be unimportant in the bimolecular S_N2 reactions of *RX* when *R* is methyl or ethyl". On the contrary, it "indicates" what it states, namely, that the total structural effect on rate when *R* = *neo*pentyl is in fact far greater than in the other cases, that Hughes could not see why the polar effect of methyl should take such a sudden jump as the last methyl is introduced, and that he could see a reason why its steric effect might do so.

Concerning Dr. Evans's historical comment that, although Hughes first estimated[3] the steric compressions involved in S_N2 reactions of *RX* for *R* = Me, Et, he did not then (1941) "specifically" consider *i*-Pr and *t*-Bu, it may be remarked that, in the approximation to which we have hitherto worked, the specific consideration which was then lacking consisted only of multiplying by 2 and 3 in the respective cases, that is, duplicating and triplicating the ethyl compressions.

We thought we had previously made it clear that we were aware of the steric effect of the α-methyl group in the bimolecular reactions of α-methylallyl chloride, but regarded Dr. Evans's method of analysis as unsound[3].

As to unimolecular reactions, Dr. Evans's method of treatment appears to be similar to that which led Baughan, Evans and Polanyi[4] to the conclusion, which we believe to be incorrect, that the known inactivity of *neo*pentyl halides in certain substitutions is a property of the ionization mechanism. Hughes established a contrary conclusion[5] in 1941.

Note added in proof. The Editors of *Nature* have allowed us to see Dr. Evans's comment. His new quotation, like the previous one, is divorced from its context: in both, the standard of comparison was the very special group, neopentyl. It should be impossible to construe such comparisons between groups as comparisons of polar and steric effects in the same (ordinary) group, since, in the paragraph now partly quoted, calculations are reported which show appreciable steric compressions in ordinary groups.

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C. K. Ingold

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[1] Evans, A. G., *Nature*, **158**, 586 (1946); cf. *ibid.*, **157**, 438 (1946); cf. also Hughes and Ingold, *ibid.*, **158**, 94 (1946).
[2] Hughes, *Trans. Farad. Soc.*, **37**, 621 (1941).
[3] Hughes and Ingold, ref. 1.
[4] *Trans. Farad. Soc.*, **37**, 377 (1941).
[5] Ref. 2. Cf. Dostrovsky, Hughes and Ingold, *J. Chem. Soc.*, 173 (1946).

I submit that the interpretation given above by Profs. Ingold and Hughes for the passage which I quoted[1] is not made clear in the original text[2]. What I see expressed in the paper regarding the bimolecular reactions of *R*-Hal for the Me, Et, *n*-Pr, *i*-Bu, neopentyl series can be illustrated once more by Prof. Hughes's statement that "It is therefore concluded that in addition to the retarding influence of methyl substitution, which operates throughout the series, and may be attributed essentially to the inductive effect, there is a superposed effect, which may in principle enter in all stages of β-substitution, but which assumes real importance only in the case of the neopentyl halide. This effect is believed to be of steric origin[3]."

Apart from this historical question, I wish to emphasize again that, in my view, the decrease in bimolecular reaction-rate along the Me to *t*-Bu series is due solely to the increase in steric hindrance along this series. This point of view is developed in detail in my forthcoming papers on the subject[3].

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[1] Evans, A. G., *Nature*, **158**, 586 (1946). Cf. Evans, A. G., *ibid.*, **157**, 438 (1946). Hughes and Ingold, *ibid.*, **158**, 94 (1946).
[2] Hughes, *Trans. Farad. Soc.*, **37**, 623 (1941).
[3] Evans, A. G., *Trans. Farad. Soc.*, in the press. "The Reactions of Organic Halides in Solution" (Manchester University Press, 1946).

A Method of Using Marked Phase Boundaries

When iron is oxidized in air, foreign particles on the original surface of the metal will later be found imbedded in the oxide layer. From this observation Fischbeck[1] concluded that iron, not oxygen, migrates through the reaction product. Starting from this fact, we have worked out the following simple method for ascertaining the direction of migration in reactions between solid substances.

On the original phase boundary a mark is made, using a substance which can be seen (at least with a microscope), does neither lessen the contact between the reaction partners nor react with them, does not act on the reaction product and does not change in any way, physically or chemically, during the process. It should, therefore, be observable after the reaction. The migration of matter is then recorded directly from the position of the mark in relation to the starting materials and the reaction product.

The method has been found suitable when studying the systems magnesium oxide/magnesium pyrophosphate[2], lead oxide/lead silicate[3], lead oxide/lead sulphate[4] and zinc oxide/aluminium oxide[5], using platinum for marking; and the system silver/sulphur[6] using gold.

Robert Jagitsch

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Chalmers Institute of Technology,
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Dec. 4.

[1] Fischbeck, K., *Metallwirtsch.*, **14**, 733 (1935).
[2] Jagitsch, R., and Peristrom, G., *Arkiv Kemi, Min., Geol.*, **22A**, No. 5 (1946).
[3] Jagitsch, R., and Bengtson, B., *Arkiv. Kemi, Min., Geol.*, **22A**, No. 6 (1946).
[4] Jagitsch, R., and Mattsson, B., unpublished results.
[5] Bengtson, B., and Jagitsch, R., *Arkiv Kemi, Min., Geol.*, in the press.

An Optical-Acoustic Method of Gas Analysis

DR. M. VENGEROV has described a method of gas analysis under this title[1]. As the method has been in practical use in the chemical industry for about ten years, I should like to direct attention to the self-recording infra-red absorption apparatus, called 'Uras', developed by Dr. Luft-Oppau.

The principle of this analyser is the same as that of Dr. Vengerov, but the arrangement is somewhat better. The radiations of two infra-red sources pass into two gilded glass tubes, after being interrupted periodically by a revolving disk with two sectors, and are absorbed finally in a condenser chamber separated into two parts by a thin metal foil. One glass tube contains a standard gas, and through the other streams the gas to be examined. The condenser chamber contains some of the gas under examination. Both the tubes and the condenser chamber are sealed with rock-salt windows, allowing the infra-red rays to pass through. In front of the separating foil in the condenser chamber is an insulated wire gauze arranged to form a kind of condenser microphone.

If the streaming gas contains an unknown component, this will absorb its characteristic radiation out of the infra-red beam. As the condenser chamber contains the same gas, it also will absorb only this characteristic radiation. The effect will be that the part of the condenser chamber beneath the analyser tube is not so much heated as the standard part, and the condenser foil is deflected less in relation to the wire gauze. The condenser microphone is in the grid circuit of a three-stage amplifier tuned to the frequency of the revolving disk.

The arrangement of two separated beams eliminates any effect of changes in the infra-red source, and the filling of the condenser chamber with a special gas guarantees great selective efficiency of the apparatus. If necessary, tubes containing the disturbing gas components can be placed in front of the analyser tubes. These tubes then act as spectral filters and absorb the characteristic radiations of the extraneous gases out of the two beams. By this device the presence of one of these disturbing gases in the gas to be examined causes no effect on the condenser chamber and the condenser itself.

With the 'Uras' the percentage can be recorded of any organic or inorganic gas having absorption bands within the range of the infra-red source, to which the condenser chamber has been adjusted. The sensitiveness is sufficient to record traces of the gas of the order of magnitude of fractions of a thousandth part.

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[1] *Nature*, **158**, 28 (1946).

Experimental Data and 'Sufficient' Accuracy

MR. H. A. HUGHES has pointed out[1] that replication could cease when a required standard of accuracy is attained, and that, from a sample size of n, the estimate, α, of the probable error of its mean ". . . decreases with ascending values of n until the minimum value permitted by the experimental conditions is reached".

Since $\alpha \sqrt{n}$ in general converges in the statistical sense to a finite limit—the probable error of a single observation—clearly α similarly converges to zero. Expressed practically, we may determine a quantity as accurately as we please by adequate replication, provided our measurements are unbiased. The elimination of bias is not, normally, possible. While, therefore, we may reduce α indefinitely, the labour of further replication is unwarranted once the magnitude of α has been reduced to the same order as that of the bias.

A second point: as Mr. Hughes referred to measurements on several systems, I infer he may ultimately have intended to compare them. If this were his sole object in attaining a prescribed accuracy, it would be most economic in replication to apply a sequential test[2]. When sequential tests are impracticable, as in some agricultural and building research when each experiment takes many months, it is possible to plan *a priori* to attain a required certainty of avoiding inadequate replication. It is intended to publish a paper on this in the near future, though it is suspected that reports, probably in the agricultural field, must already exist of prior research on this topic.

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Ministry of Works.
Dec. 2.

[1] *Nature*, **158**, 29 (1946).
[2] Wald, A., *Ann. Math. Stat.*, **16** (1945).

Fluid Flow at a Small Hole due to Vibration

I SHOULD like to acknowledge my indebtedness to Prof. Andrade[1] for directing my attention to the work of H. Sell[2]. As a result, I have made a careful examination, by means of strong lateral illumination, of the way in which the smoke particles enter and leave the orifice in the resonator. There is no doubt about the general accuracy of H. Sell's explanation, and this is particularly well demonstrated when the smoke is made to issue in the space between two parallel plates of glass kept apart by a distance piece the thickness of which is approximately that of the diameter of the orifice. The smoke particles can then be seen as bright dots on a dark ground, and their paths easily traced. As well as the back-flow near the surface of the resonator, vortex motion occurs. If, however, the smoke be allowed to issue from a V-shaped mouthpiece formed by sealing two flat pieces of glass between the plates, then, provided the excitation is not too powerful, both the vortex motion and the back-flow cease. There now occurs what appears to the eye as a slow steady motion of the smoke outwards. Examination of the particles with a low-power microscope shows them, however, no longer as dots but as little lines, so that a vibratory motion is superimposed on the general forward motion.

It would appear, therefore, at first sight, that the experiment is now on a more satisfactory basis. When the resonator is constructed of glass, however, vortex motion *inside* the tube in the neighbourhood of the orifice can be seen. Particles within the tube travel normally away from the centre of the orifice, pursue a more or less circular path and return along the base to the orifice once more. Some of these particles, however, move outwards through the orifice, and these constitute the slow steady stream

already referred to. The phenomenon is thus of a complicated nature which it is hoped further to investigate.

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[1] *Nature*, **159**, 132 (1947).
[2] Raleigh, "Sound", **2**, 202, describes a somewhat similar phenomenon.

A REPETITION of the demonstration described by Prof. G. D. West in *Nature*[1] showed that it works particularly well with a wide test-tube of length suitable for a 256-fork (even the small laboratory pattern), and having at the closed end a central hole of diameter less than 1 mm., shaped rather like the small aperture of a Helmholtz resonator. By performing the experiment with the aid of a projection lantern, both this effect and a reverse effect not mentioned by Prof. West can readily be shown to a large class. On bringing the tuning fork near the open end of the tube, a puff of smoke is seen to issue from the hole in the closed end, followed by a steady stream of smoke so long as the vibrating tuning fork is held in position. Under the same conditions, a puff of clear fresh air can be seen to enter the tube from outside, followed by a steady stream of fresh air. This is precisely what one would expect, for the closed end of the tube is a pressure antinode, and the so-called 'steady pressure' of sound is in reality a differential effect in which the compressional phase preponderates over the rarefactional phase, the alternating emergence and intake of gas observed at the orifice taking place so rapidly that the eye sees what appears to be a steady stream of gas flowing simultaneously in both directions at the orifice.

The existence of a pressure antinode at the closed end of a resonance tube can also be clearly demonstrated by an arrangement devised by me in 1937 and hitherto unpublished. A glass tube is closed at one end by a clear glass disk about 1 mm. thick. The open end of this tube is attached by 'Plasticene' to the mouth of a loud-speaker (disk type) actuated by an oscillator of variable frequency. The glass disk is found to vibrate perceptibly for certain frequencies of the oscillator, and if a long-focus convex lens is attached by wax peripherally to the glass disk, the resonant states in the tube can be detected by observation in sodium light of the disappearance or enfeeblement of the Newton's rings formed between the lens and disk. With this arrangement, up to eight harmonics of the fundamental frequency of the tube can be detected.

Quite recently another simple device has been found to be very sensitive for the detection of resonance and the existence of an antinode of pressure at the closed end of a cylindrical tube. The closed end is formed by attaching a thin 'Cellophane' sheet to one end of the tube (a 'Cellophane' jam-pot cover serves admirably). The 'Cellophane' is applied wet, and on drying it is quite taut and vibrates vigorously when a suitable tuning fork is held opposite the open end of the tube. If a small quantity of silver sand is dropped on to the 'Cellophane' membrane and the tube is of variable length, the adjustment for resonance can be accurately determined, either by the intensity of the clearly audible patter of the sand on the membrane, or visibly by observation of the movement of the sand. This effect is not to be confused with the corresponding effect in which sand on thin tissue paper attached to a ring is used for detecting antinodes of motion in an organ pipe. The sand on 'Cellophane' detector can be employed effectively in the lantern to demonstrate resonance, when the motion of the sand can be seen and the patter of the sand is audible even in a large lecture room.

In conclusion, it may be mentioned that any of these methods can be used for demonstrating the existence of the 'interference hyperbolæ' around the end of a tuning fork, for the effects described cease as soon as the interference regions fall on the axis of the tube. Either the Newton's rings detector or that using 'Cellophane'-sand can be used with advantage in a laboratory method for the determination of the velocity of sound. The latter detector has been applied successfully for this purpose in this laboratory by my colleague, Dr. E. L. Yates. In the original form of the apparatus, one end of the tube was closed by a loud-speaker element operated by an oscillator at fixed frequency (1,600 cycles per second). The receiver was a hollow cylinder fitting closely in the tube and closed at the near end by a thin copper membrane. The other end of the cylinder was connected to a straight brass tube ending in a rubber listening tube. By noting the position of an indicator on the brass tube for a number of successive resonances, the wave-length of the sound waves could be determined, and hence the velocity. In its modified form, the bottom end of the glass tube is closed by a 'Cellophane'-sand detector and the loud-speaker element (deaf-aid type) is attached to the end of the brass tube. With this arrangement the wave-length can be determined with much greater accuracy, the audible patter of the sand giving sharp response at resonance when the oscillator is operating at minimum output.

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Dec. 24.

[1] *Nature*, **158**, 755 (1946).

"The 'Squares' Method for Potential Problems"

THE review by Prof. G. Temple in *Nature* of November 23 is more than generous to my recently published book on "Relaxation Methods", and with my fellow-workers I am grateful for his praise; but a slip in its penultimate paragraph will, I fear, cause misunderstanding if it is not corrected. It is not the relaxation technique but the technique employed, for example, by Shortley and Weller of Ohio State University (who have attributed it to Liebmann, 1918) that "consists in systematically working over the lattice and everywhere replacing the original approximate values of f by the means of its values at the neighbouring corner points".

For brevity this systematic process may be described as 'scanning', since Wiener (among others) in the United States has suggested that the methods of television might be employed to speed up the mean-value computations. It finds no place in the technique of relaxation methods, in which (excepting in the final presentation of results) values of f are *not* recorded at lattice points. Instead, the values recorded are those of 'residuals' defined (in Prof. Temple's notation) by

$$F_P = f_A + f_B + f_C + f_D - 4f_P$$

(or by convenient multiples), and the F's are brought to negligible values ('liquidated') by operations which have nothing in common with 'scanning', and are not performed in an iterative sequence.

It was on this account (I suppose) that Prof. H. W. Emmons wrote in the *Quarterly of Applied Mathematics* (October 1944): "In fact, for the computer (as opposed to those who think only about the logic behind the computation methods) the relaxation method has a spirit lacking entirely from the iteration process. The former challenges one's intellect at each step to make the best possible guess, while the latter reduces one to the status of an automatic computing machine." Working throughout in terms of F's, not f's, a computer is *not* compelled to "alter the value of f only at one point at a time"; nor, when he does so, does he make that alteration (usually) what is given by choosing f_P so as to make $f_P = 0$. That rule, as Prof. Temple shows, defines the best way of altering f_P in a single operation; but economy of time demands the minimum number of operations, and from this point of view the rule can usually be bettered. Herein lies the 'challenge' to the computer.

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Economy in space compelled me to focus attention on the most fundamental and elementary process of the 'squares' method, and I could only indicate briefly in the concluding paragraph the higher flights of relaxation technique.

Readers of *Nature* will be grateful for Dr. Southwell's fuller statement of the principles and the spirit of this method.

G. Temple

Effect of Mono- and Divalent Salts on Red Blood Cells

Greville and Lehmann[1] reported that less hæmolysis occurred when human blood was stored with $M/7$ magnesium chloride as an anticoagulant than if isotonic sodium citrate was used. We have investigated this phenomenon for several reasons, one of which was its possible practical bearing on the storage of sheep blood corpuscles used for the Wassermann reaction in the diagnosis of syphilis.

Fragility Tests. When fragility tests are performed on human blood cells, it can be seen that the osmotic pressure of divalent salts, such as magnesium and calcium chloride and magnesium and sodium sulphate, is about twice that of sodium or potassium chloride (Table 1).

The cells were obtained from citrated blood, washed in the corresponding salt solution and centrifuged. When stored, they were kept at + 4° C. in $M/7$ salt solution in a concentration of 4,000,000 cells per c.mm. For the fragility test, packed cells were suspended to give a concentration of 100,000 cells per c.mm. in 3 ml. of salt solutions diminishing in concentration by 1 per cent of $M/7$ from sample to sample. The highest concentration at which complete hæmolysis was seen was noted after the tubes with the cell suspensions had been allowed to stand for 2 hours at + 4° C.

Table 1. Highest salt concentrations at which human red blood cells are completely hæmolysed. Two typical experiments. Concentrations of salt solutions tested diminishing by 1 per cent of $M/7$

Experiment	KCl	NaCl	$MgCl_2$	$CaCl_2$	Na_2SO_4	$MgSO_4$
A	*M*/24	*M*/23	*M*/46	*M*/44	*M*/46	*M*/50
B	*M*/26	*M*/25	*M*/58	*M*/50	*M*/48	*M*/46

Storage of cells in solutions of divalent salts was seen to alter the properties of the cell surface. Compared with $M/7$ sodium chloride, in $M/7$ magnesium or calcium chloride red blood cells became more brittle. They were more easily destroyed by mechanical trauma such as centrifuging, but, at the same time, they became more resistant in hypotonic salt solutions. A similar observation was made by Aub *et al.*[2] when they treated red cells with small quantities of lead. The phenomenon can be observed particularly well in sheep cells, which due to a slight difference in shape from human cells are more easily broken up in hypotonic solutions.

Table 2. Highest concentrations of sodium chloride at which sheep red blood cells are completely hæmolysed after previous storage in $M/7$ solutions of sodium and magnesium chloride

Storage	None	2 days		9 days		16 days	
Salt used for storage	—	NaCl	$MgCl_2$	NaCl	$MgCl_2$	NaCl	$MgCl_2$
Highest NaCl concentration showing complete hæmolysis	*M*/14	*M*/13	*M*/17	*M*/13	*M*/28	*M*/14	*M*/39

It will be seen from Table 2 that the highest concentration of sodium chloride at which sheep red blood cells were completely hæmolysed was the same whether the cells were tested immediately after they were obtained, or whether they had been stored in $M/7$ sodium chloride; storage in $M/7$ magnesium chloride, however, increased the resistance against hypotonic sodium chloride solutions, and the 'toughening' of the cell membrane became more noticeable the longer the storage.

Sensitivity to Hæmolysin. When sheep cells were hæmolysed by antibodies from sensitized rabbits, there was no difference seen when the corpuscles used were stored in $M/7$ sodium or magnesium chloride. The extent of hæmolysis was varied by the addition of various amounts of complement (Wassermann reaction), but the degrees of lysis were the same however long and in whatever isomolecular salt solution the corpuscles had previously been stored (sixty-four experiments).

Table 3. Average mean corpuscular volumes of ten samples of human red blood cells suspended in various salt solutions
Average mean corpuscular volume in plasma: 86 ± 8 cubic μ.

Salt concentration		KCl	NaCl	$MgCl_2$	$CaCl_2$
M/7	cubic μ	101 ± 5	105 ± 6	80 ± 15	77 ± 20
M/14	cubic μ	124 ± 4	131 ± 6	107 ± 5	104 ± 6

Corpuscular Volume Measurements. Table 3 shows the average of the mean corpuscular volumes found when ten different samples of human red cells were suspended in solutions of potassium and sodium chloride and of magnesium and calcium chloride respectively. Sodium and magnesium sulphate could not be tested as they rendered the cells so brittle that they were destroyed during the centrifuging necessary for the determination of the packed cell volume, and deteriorated even when shaken in the diluting pipette prior to transfer to the counting chamber.

The cells were obtained from plasma oxalated after Wintrobe, washed in the corresponding salt solutions and centrifuged. Packed cells were added to 2 ml. of salt solution to give a final concentration of 2,000,000 cells per c.mm. The suspensions were left for 15 hours at + 4° C., and the mean corpuscular volume was then determined by dividing the packed-cell volume percentage by the cell concentration.

It will be seen in Table 3 that the same size is taken by corpuscles in an $M/7$ solution of monovalent salt as in half that concentration of divalent salt.

It is noteworthy that the cells increase in volume when transferred from plasma to 'normal saline' ($M/7$ sodium chloride); this is due to the absence of the osmotic pressure of the plasma proteins. This considerable influence of the plasma proteins on the corpuscle-size may underlie the observations of Dyson *et al.*[3] and of Walters *et al.*[4]; the former observed increase of corpuscle-size in blood-loss where tissue water replaced the lost plasma protein, the latter saw shrinking of corpuscles when malnourished individuals received plasma transfusions. Whereas the cells swell in size when transferred from plasma to normal saline, Table 3 shows that they retain their original volume when suspended in $M/7$ solutions of divalent salts. This fact, as well as the 'toughening' influence of divalent ions on the cell membrane described above, may explain the comparative stability of red blood cells observed by Greville and Lehmann[1] when they used $M/7$ magnesium chloride as an anticoagulant.

Our thanks are due to the director of Medical Services in India for permission to publish this communication.

H. Lehmann
J. O. Laws
N. Ahmed

Eastern Command Laboratory,
Namkum, Ranchi,
India Command.
Nov. 28.

[1] Greville, G. D., and Lehmann, H., *J. Physiol.*, **103**, 175 (1943).
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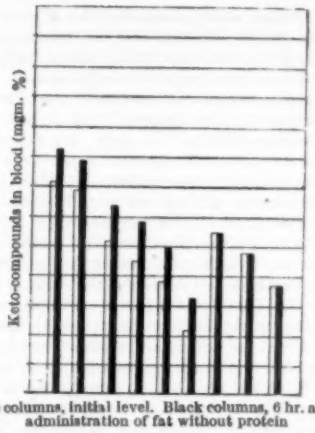
Effect of Proteins on Ketonæmia

The presence of fat is necessary for the increased production of ketone bodies, because they originate from fat. But the part played by the proteins is undecided. Some of the amino-acids are ketoplastic, while others act as antiketoplastic. According to Schaffer[1], fifty-eight per cent of protein is of antiketoplastic effect, while forty-two per cent is ketoplastic; carbohydrates are antiketoplastic in their entirety, while ten per cent of the fats (glycerol) are antiketoplastic, and ninety per cent have a ketone-producing effect. According to Schaffer, ketosis ensues if the amount of ketogene molecules outnumbers that of antiketogenic ones. But Falta[2] could not confirm this attractive explanation. He is of the opinion that proteins exert a specific ketogenic effect, which is not only dependent on the amount of ketoplastic material contained in them; for if he fed much fat and little protein to diabetics, acetonuria



White columns, initial level. Black columns, 6 hr. after administration of fat without protein

was less than if he did the reverse. But this opinion is not shared by such eminent clinicians as Porges and Adelsberg[3] and Noorden[4].

The discussion proves that the part played by the proteins in the origin of ketosis is still doubtful, and so the question whether they cause ketosis in ratio to their content of ketoplastic components, or a specific ketoplastic effect of their own beyond that, is still undecided. Further elucidation can only be expected by quantitative investigation, and so far opinion has depended almost exclusively upon clinical observation. Therefore, we undertook the experiments described below.

By the bisulphite-binding method of Clift and Cook[5], we determined the ketone bodies of diabetic and non-diabetic patients in the morning on an empty stomach. Then 150 gm. of oil was administered by stomach-tube (in one case 120 gm. of bacon was ingested) and after six hours the determination was repeated. In another series, fat (120 gm. of bacon) and proteins (in the form of 100 gm. of cheese) were fed.

The table below demonstrates that increase of ketone bodies is insignificant after fat alone; three cases showed no increase at all. This result corresponds to that of Leites, Odinow and Powolowskaja[6], who after ingestion of fat found a ketogenous effect only in some of their cases, while Hoffmann and Anselmino[7] reported an increase in every case. Our results are also in accordance with the findings of Marks and Young[8]. They found that ketonuria was highest in dogs with meta-hypophyseal diabetes when the animals received a diet of raw meat, and lowest with a high-fat diet. Similar results have been obtained by Burn, Lewis and Kelsey[9] with alloxan-diabetic rats.

If fat and protein were administered together, a much more considerable increase followed, with the exception of cases of pregnancy[10], as would have been expected, in proportion to the ketoplastic amino-acid content of the protein. The ketogenic component of proteins being on the average forty-two per cent, after ingestion of fat and protein an increase of one sixth would be expected at the most above fat alone. In our experiment 100 gm. of cheese was ingested besides 120 gm. of fat; the protein content of the cheese being approximately twenty-five per cent. But while after fat the increase amounted on the average to 18·3 per cent, that following combined fat and protein intake averaged 65·2 per cent. If

the increase were determined by the ketogenic component exclusively, the ultimate value should be only one sixth larger, that is to say, twenty-one per cent on the average. The difference was statistically significant.

In two cases the blood of the same patients was examined before and after intake of fat, and before and after intake of fat and protein.

INCREASE OF KETONE COMPOUNDS IN BLOOD

	After fat and protein	After fat alone
Diabetes	43 per cent	0 per cent
Cirrhosis of liver	49 per cent	16 per cent

These two cases clearly show the reliability of the conclusion drawn from averages of all cases investigated. In both cases a considerable increase is evident after ingestion of fat and protein; whereas one of them shows no increase at all after fat alone, and the other a slight increase, corresponding to the average of all cases.

Summary. After ingestion of fat and protein, the blood shows a considerably increased content of ketone bodies as compared to fat ingestion alone. This exceeds significantly the amount which could be ascribed to the content of proteins of ketogenic amino-acids, and therefore it may be assumed that proteins exert a specific ketoplastic effect.

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Nov. 25.

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Treatment of Keratitis rosacea with Small Doses of Testosterone

IN the course of some investigations on 'endocrine allergy'[1], it was found that patients with keratitis rosacea (six cases) give a positive intracutaneous test to testosterone. Tests with a series of several other steroid hormones, on the other hand, were negative. Testosterone administered intracutaneously in increasing doses for the purpose of desensitization brought about a striking improvement of the ocular condition in all the patients. In cases in which the keratitis rosacea was associated with facial rosacea, a distinct improvement was also observed in the latter condition.

Although therapeutic results in keratitis rosacea must be interpreted cautiously in view of the occurrence of spontaneous remissions, allergy to testosterone appears to be a cause of this condition in at least a high percentage of cases. A detailed study will be published in the *British Journal of Ophthalmology*.

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Development of *Ornithodorus moubata* on Normal and Thiamin-deficient Rats

De Meillon and Golberg[1] reported that early nymphs of *O. moubata* when fed on thiamin-deficient rats display a retardation of growth as compared with nymphs fed on rats given a full diet. Since that time, the ticks have been reared to maturity and the following results obtained.

DEVELOPMENT OF *O. moubata* ON NORMAL AND THIAMIN-DEFICIENT RATS

Stages of development		Rats fed on Full diet	Rats fed on Thiamin-free diet
I	No. fully fed	20	24
II	*P*	96	100
	T	9·2 ± 0·4	11·6 ± 1·4
III	*P*	100	90
	T	8·6 ± 0·7	14·0 ± 1·2
IV	*P*	100	100
	T	8·8 ± 1·4	12·5 ± 1·7
V	*P*	100	78
	T	14·5 ± 1·0	26·4 ± 12·0
VI	*P*	—	64
	T	—	15·2 ± 0·8
Mean time (in days) to adult stage		41·1	79·7
Number of moults to reach maturity		4	5

P, percentage of fully fed ticks which moulted.
T, mean and standard deviation of time (in days) from feed to moult.

The evidence presented in the table shows that thiamin deficiency in the host affects the development of *O. moubata* by prolonging the time taken between each feed and the subsequent moult, and by inducing an additional moult before maturity is reached. As a result, the ticks on thiamin-deficient rats take almost twice as long to become adults as do ticks fed on normal rats. Unfortunately, all the ticks from the control group of nymphs were females, so that it is not possible to compare egg production in the two groups.

We have now shown that two unrelated blood-sucking arthropods, *Cimex lectularius* and *O. moubata*, are adversely affected by feeding on thiamin-deficient rats. Experiments with other deficiencies are under way.

We wish to thank Miss F. Hardy, Miss V. B. Arvidsson and Mr. J. M. Thorp for technical assistance.

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Seasonal Variations in Vitamin C Content of Tomatoes Grown in Great Britain

RESULTS obtained in the Ovaltine Research Laboratories in 1942[1] suggested that the vitamin C content of tomatoes grown in Great Britain is affected by the degree of exposure to sunshine during cultivation. It is also related to the size of the fruit, position on the plant, date of collection, etc.; but if these and other disturbing factors are balanced by careful sampling, the average vitamin C content might be expected to vary significantly with sufficiently wide differences in climatic conditions in different seasons.

These suggestions have been supported by experimental results obtained at Long Ashton in 1944[2] and 1945[2]. The average vitamin C content of tomatoes grown out of doors in 1945 was only three quarters of

that of tomatoes grown under the same conditions in 1944, sufficient samples being taken to ensure a significant difference. The lower vitamin C content in 1945 was correlated with a smaller number of hours of sunshine during August, September and October. Strange to say, this latter factor made only a slight difference to the vitamin C content of tomatoes grown under glass, which in both years averaged rather less than two thirds of the average outdoor figure in 1944.

TABLE 1. SEASONAL VARIATIONS IN VITAMIN C CONTENT OF ENGLISH TOMATOES

Season	Districts where grown	No. of varieties	No. of samples	Mean vitamin C content (mgm./100 gm.*)		Hours of sunshine during cultivation†
				Cultivation indoor	Cultivation outdoor	
1942	Elstree	mixed	11	15·4	—	—
1944	King's Langley	mixed	39	—	16·6	—
	Long Ashton	2	130	19·5	—	471
		2	80	—	30·3	
1945	Long Ashton	9	49	17·7	—	347
		13	64	—	22·3	
1946	Elstree	mixed	11	15·8	—	—
	Maidstone	5	117	—	16·0	345

* Of edible portion of fresh fruit.
† Included August, September and October until harvested.

Climatic conditions in 1946 have been even worse than in 1945, and the growth and ripening of tomatoes out of doors have been more difficult than usual. Therefore, we think it of interest to compare in Table 1 average figures for the vitamin C content of tomatoes grown indoors and outdoors this year with the average results previously obtained in these conditions at King's Langley and Long Ashton. Whereas our average indoor figure (though based on only a few samples) does not differ significantly from the indoor averages obtained in previous years, our average outdoor figure is much lower than the Long Ashton outdoor averages in 1944 and 1945, though it agrees well with the outdoor average in 1942. Varietal differences cannot explain more than a small part of this difference, because one of the varieties (Premier) which we have tested this season was also tested at Long Ashton, and the ratio of its average vitamin C content to that of all the five varieties we examined was 1 : 1·3, as compared with 1 : 1 obtained at Long Ashton for all their varieties. Fertilizer treatment also will not satisfactorily explain the difference. We tried a variety of fertilizer mixtures containing known amounts of nitrogen, phosphorus, potassium and calcium, and, while some of these did appear to increase the vitamin C content, the results obtained on different samples showed that climatic conditions were still an important factor. The outdoor tomatoes we examined were grown at Maidstone Technical Institute and were of medium size (that is, 60–120 gm.), care being taken to exclude very small and very large fruit in which the difference in size might affect the vitamin C content. Hence this factor also would not seem to explain the lower average vitamin C content found this season.

In order to study the climatic conditions more closely, the mean hours sunshine daily and the mean monthly maximum and minimum temperatures were determined (Table 2). The latter also includes parallel results and averages for a number of previous

TABLE 2. SUNSHINE AND TEMPERATURE RECORDS DURING TOMATO CULTIVATION

Season	Districts where grown	Mean hours sunshine daily			Mean monthly maximum and minimum temperatures (° F.)		
		Aug.	Sept.	Oct.	Aug.	Sept.	Oct.
1944	Long Ashton	6·9	5·0	3·4	—	—	—
1945	Long Ashton	5·1	3·0	2·1	—	—	—
1946	Maidstone	5·7	3·9	3·0*	53–69	50–63	50–62*
1946	East Malling	5·7	4·2	—	51–68	50–65	45–60†
	East Malling. Mean of last 30–33 years	5·8	4·8	—	52–71	48–66	—
	East Malling. Mean of last 10 years	—	—	—	52–71	48–66	45–60†

* Until final harvest.
† First fortnight.

years at East Malling, near Maidstone (kindly supplied by the Director of the East Malling Research Station). Although there is much evidence that the vitamin C content is affected by exposure to sunshine, and possibly also by warmer climates, the data do not provide clear-cut proof of this, and it is to be hoped that other workers who have been examining tomatoes this season will also compare the effect of climatic factors. We have confirmed previous findings that the vitamin C content is little altered by ripening indoors off the plant instead of outdoors on the plant.

Our results have been obtained on fruit cultivated, collected and stored with greater precautions to increase and conserve vitamin C than can normally be applied to commercial tomatoes, and the figures given in Table 1 for the average vitamin C content must be appreciably higher than the average vitamin C content of similar tomatoes as purchased by the public. It is, therefore, of interest to notice that the average vitamin C contents quoted in this table are considerably lower than the figures of 7 mgm./oz. and 25 mgm./100 gm. recently published from the Ministry of Food[3] and the Medical Research Council[4] respectively. It would be desirable to know how the official figures were obtained, and what allowance was made for climatic and other factors affecting vitamin C content, before deciding whether they can be used as a fair indication of the vitamin C content of British tomatoes in 1946.

We are indebted to Miss E. Finnegan, Miss H. Williams and Miss J. Wyatt for assistance in preparing the extracts and carrying out the estimations.

Full details of these results will be published elsewhere.

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FUTURE OF THE SOCIAL SCIENCES

A CONFERENCE of social scientists, convened by the Association of Scientific Workers, was held on December 14 at Gas Industry House, London, S.W.1. Mr. J. R. M. Brumwell took the chair, and the chief speakers were Prof. S. Zuckerman, Dr. G. Wagner, Mr. Dennis Chapman and Mr. R. Innes. The chairman recalled the meeting held in October 1945, at which a Joint Sciences Committee had been set up to foster relations between workers in the natural and social fields of science. The Committee had functioned successfully, and, as a direct result of its activities, an increased awareness of the links between natural and social science had spread throughout the Association of Scientific Workers. In August 1946, the functions of the Committee were assumed by the Science Policy Committee of the Association. This step was taken to avoid duplication of effort because the work of the two groups soon began to overlap. The Joint Sciences Committee was provisionally reconstituted as a Social Sciences Committee to concentrate on problems of organisation and development of the social sciences themselves. Social scientists in the Association had become more conscious of their own problems of recruitment and training. It was now proposed to set up a national section of the Association of Scientific Workers representing social scientists throughout the United Kingdom.

The terms of reference of this group were: first, to advise on policy in regard to the social sciences; second, to advise on conditions of work and employment of social scientists; third, to stimulate the recruitment of social scientists into the Association; and fourth, to suggest ways of ensuring adequate representation of social scientists on committees of the Association.

Prof. Zuckerman spoke of the value of scientific discipline in social affairs and in the objective study of human institutions. He quoted with approval some observations made by Mr. Herbert Morrison in his Hobhouse Memorial Lecture, which revealed a sympathetic appreciation of the potentialities of social science. He went on to discuss the Clapham Report and, while admitting that the Report suffered from certain shortcomings, maintained that they were not so serious as some of its critics seemed to suppose. The Clapham Report marked a step forward though, possibly, a short step and not the great stride desired by the critics. It was the first official recognition of the existence of the social field of research. In Prof. Zuckerman's view the chief obstacle to progress in this field lay in the shortage of trained research workers. Hence he stressed the need for establishing a body to determine priorities for pursuing research in the social sciences. In the present state of knowledge such a body could perform a more valuable service than a Social Science Research Council, if such were set up.

Prof. Zuckerman drew a distinction, in the light of his war-time experience, between the tactics and strategy of social operations. Operational research during the War had its roots in the need to solve tactical problems as exemplified in the field of radar. But as the War advanced, the work of operational research stations, in the Army and Navy as well as in the Royal Air Force, began to impinge upon problems of strategy as well. The needs of peace likewise opened up possibilities of a dual tactical and strategical approach. Tactical problems arose, for example, in the fields of traffic, output per worker, management, incentives, selection of personnel, and building processes, all of which required investigation 'on the spot'. He remarked that in connexion with the building programme, research of this type was proceeding apace, and it was possible to control the chief variables. But such problems should be distinguished from the more fundamental strategical problems involving the selection of alternative policies or courses of action. The task of scientific workers was not merely to 'clean-up' problems arising out of a situation not of their own making, but to play a part in creating the social situation. As yet, natural sciences had not become involved in the strategy of social operations; but research which commenced with a tactical aim might acquire a strategic value. Thus Prof. Bernal's studies of air raid effects began with the aim of assessing the protective properties of shelters. They developed into a basis for offensive strategy by providing data on the dislocation of social and industrial life resulting from air attack. In this way, scientific method came to influence the choice of a major line of policy. In peace-time, economists, in particular, had an important strategical part to play, for policy might be directly determined on the basis of the data provided by them. In the epoch of planning into which we were entering, science had an important strategical function to perform.

Research in the social sciences was, in his view, expensive as, for example, in carrying out social surveys. With the present shortage of trained manpower, however, and the organisational difficulties, it was, in fact, difficult to spend large sums of money on research. Housing surveys were now 'limping along' for lack of trained workers to carry them out. There was an urgent need for more social surveys to illumine policy.

An important factor in stimulating the development of the social sciences was the attitude of administrators. It was essential that administrators should acquire an appreciation of the use of scientific method in human affairs. Experience in operational research had taught that a genuine scientific attitude on the part of the investigator mattered more than training in a particular field of science. On occasion, arts graduates without a specific training in science had proved their worth in this field.

The future for the social sciences, he went on to say, must be viewed in the light of the academic background, the number of trained workers available, the adequacy of the methodology, the various fields of application and the opportunities for *ad hoc* tactical and fundamental strategical approaches.

In summing up, Prof. Zuckerman concluded that the outlook for the social sciences was 'very rosy'. Authorities in the field of social action were more ready now to listen to the advice of men of science. Nothing could therefore prevent further expansion.

Mr. R. Innes outlined the organisation of the Association of Scientific Workers. There were seven national sectional committees, including, for example, chemistry and engineering and metallurgy. It was proposed that the terms of reference of the provisional social sciences committee would carry over to a new national committee. Members of the Association in different parts of Great Britain who were active in the field under consideration would be linked together through the national committee, which should be widely representative of interested members.

Mr. Dennis Chapman made a strong plea for the expansion of social research. The position of the social sciences, he said, had been transformed by the publication of the Clapham Report. There were two main fields of activity. First, there was the field of fundamental research. Here, he thought, the position was lamentably backward. Brilliant generalizations of two or three decades ago by Patrick Geddes, the Webbs, Graham Wallas and others had lain dormant for want of initiative to follow up with research. Over a wide potential range of social problems research was, at present, non-existent. No inquiries were being undertaken on such subjects as human relations in industry and politics, the family, the church and other institutions or on the social process of education. No studies were being made of the social psychology of morale, civil and industrial, or into the medical and nursing services of Britain. No one had yet attempted to study the curious distribution of civic vitality in different cities. The methods employed to-day in social research needed revitalizing. They had not advanced since the days of Charles Booth and Beatrice Webb. Many of the techniques were thirty or forty years old and needed bringing up to date. In the United States the methods of social measurement had made far more rapid progress.

In the applied field, the position was much the same. Little research was being pursued. Yet every administrative action was a social experiment and threw up problems for investigation. Social processes were constantly changing and creating experimental situations. It was untrue to assert that there were no experiments in the social field. On the contrary, social scientists were overwhelmed by the multiplicity of experiments taking place around them.

The situation as regards the status and salaries of social scientists left much to be desired. In the universities, status was good but the salary of the few engaged in the 'sweated trade' of social research was appreciably below that of a teacher with similar qualifications in the same subject. In Government Departments, on the other hand, there was no settled status for social scientists. Many were regarded as administrators or as statisticians; scope for publishing results of official research was also severely restricted. It would be disastrous, he thought, if all financial support for social research went to the universities. Almost every important advance in social research, so he declared, had taken place outside the universities. There was a strong case for setting up a Social Science Research Council.

During the ensuing discussion, a number of important points came to light. In Government Departments, it was said, personnel work failed to be accorded adequate recognition. It was a neglected service to be run by officers of the clerical grade without appropriate training. Another speaker challenged Prof. Zuckerman's assumption that scientific method influenced administrative decisions in sufficient measure; a great deal more 'pre-decision' analysis was needed. In this connexion, he mentioned that the Social Sciences Committee had set up a special group to study the requirements of a scientific public administration.

A third speaker reminded the meeting that it was not the function of the Association to carry out research for which full-time investigators properly financed were required. But the Association could survey a field so as to indicate the kind of research and scientific organisation that were needed. What the Association had attempted to do for fuel and power could be done for the field of social inquiry. The main weakness at the moment was a deficiency in fundamental knowledge of social phenomena. He did not regard as adequate the suggestion in the Clapham Report that provision for social research in the universities should be determined by a sub-committee of the University Grants' Committee. Opportunities should be left open for the talented individual with an inquiring mind. Universities, Government departments and other institutions should not monopolize research. Anyone who had ideas for research and could convince a small authoritative body of their value should be given sufficient financial aid to carry out the necessary inquiries. If such steps were taken the large number of workers with experience in operational or social research would have an opportunity of proving their worth. This speaker also directed attention to the need for studying the effects of advances in the social and economic research upon the development of technique in the physical sciences. We were well aware of the impact of science on society. What was now needed was a consciousness of the reciprocal influence.

Several speakers criticized the tacit assumption of the Clapham Report that all social research was expensive. It was often implied, it was said, that most social inquiries involved questionnaires on a large and costly scale. The need for 'penetration' studies of the individual, of the kind made in social anthropology, tended to be overlooked. Measurement was not a *sine qua non* of social research. There was a complete absence to-day of really fundamental social research, with the possible exception of industrial psychology.

Before the meeting drew to its close, a national sectional committee of the Association of Scientific Workers was set up with the terms of reference as described above, and the members duly elected.

JOHN COHEN

BRITISH CONTRIBUTION TO RADIOLOCATION

EARLY in March 1946, the Institution of Electrical Engineers held a Radiolocation Conference at which were presented a large number of lectures and papers by various members and other individuals who had contributed to the development and application of radiolocation during the war years. The Convention was formally opened at an ordinary meeting of the Institution by the Minister of Supply, Mr. John Wilmot, and was followed by a lecture on "The Evolution of Radiolocation" by Sir Robert Watson-Watt. In the course of this lecture it was explained that the object of the Convention was to describe and discuss British activities in this subject which began in February 1935 and were at first conducted independently of any similar work elsewhere. Following the co-operation established between Great Britain and the United States in the autumn of 1940, later developments were conducted on the basis of a United Nations' system for which the name 'radar' was adopted in 1943, this term being then in current use in the United States.

The opening meeting referred to above was followed by twelve special meetings of the Radio Section of the Institution devoted to lectures and

papers on special portions of the technical field of development and their applications. The wide range of subjects dealt with is indicated by the titles adopted for these meetings: "Aerials and Wave Guides"; "Propagation"; "Cathode Ray Tubes"; "Precision Radar"; "Valves"; "Radio Measurements and Test Gear"; "Shipborne Radar"; "Transmitters and Receivers"; "Circuit Techniques for Radiolocation"; "Radar Navigation" (two meetings); and "Naval Gunnery Radar". At most of these meetings one or two papers were first read, giving a general survey of the portion of the field under discussion; and these were followed by a series of brief lectures by experts on particular sections of the subject. Three of the meetings were each devoted to a formal lecture on a phase of the subject to which special attention had been given by the Army, Navy and Air Force respectively. The authors of all these contributions, the total number of which exceeds 160, included members of the staffs of various Government establishments, industrial organisations and universities, who had co-operated in the very large amount of research and development work undertaken during the war years.

The *Proceedings* of this Radiolocation Convention are now being published as nine special numbers of Part 3 of the *Journal of the Institution of Electrical Engineers*, of which the first two, Vol. 93, Part 3 A, Nos. 1 and 2, have recently been issued. The first number comprises a basic record of all the meetings described above, the survey papers and some of the lectures being published in full; while abstracts of the remaining lectures and of the supporting papers, the full texts of most of which will appear in later numbers, are also given, together with some sectional bibliographies of other supporting papers to be published later. This publication thus forms a most useful work of reference to the whole literature so far available describing the British contribution to the technical development of radar or radiolocation.

The second number (Vol. 93, Part 3 A, No. 2) of this special issue of the *Journal* contains the full text of the six papers read at the session on "Naval Gunnery Radar", of the four papers read at the session on "Radar Navigation", and of two papers dealing with certain aspects of the technique of "Precision Radar". This publication will be found of great use to those responsible for detailed research and technical development in these subjects, of which radar as an aid to both aerial and marine navigation is already well advanced in its civil applications.

FORTHCOMING EVENTS

Monday, February 3

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 5 p.m.—Dr. A. Dickson Wright: "The Possibilities of Recent Discoveries in Medical Diagnosis and Treatment" (Cantor Lectures, 2).

SOCIETY OF ENGINEERS (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Inaugural Meeting for 1947. Mr. Victor S. Wigmore: Presidential Address; Mr. A. E. Turner will present the Premiums awarded in 1946.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY (in the Reynolds Hall, College of Technology, Manchester), at 5.30 p.m.—Mr. Norman Pye: "Hyperbolic Navigational Charts".

SOCIETY OF CHEMICAL INDUSTRY, FOOD GROUP (joint meeting with the LONDON SECTION, at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 6.30 p.m.—Dr. E. Griffiths and Mr. F. R. Jones: "Refrigeration—Development and Application".

Tuesday, February 4

INSTITUTION OF CHEMICAL ENGINEERS (joint meeting with the ROAD AND BUILDING MATERIALS GROUP OF THE SOCIETY OF CHEMICAL INDUSTRY, at the Institution of Structural Engineers, 11 Upper Belgrave Street, London, S.W.1).—Conference on "Particle Size Analysis". At 10 a.m.—"Methods of Measurement"; at 2 p.m.—"Industrial Applications".

Wednesday, February 5

ROYAL SOCIETY OF MEDICINE, HISTORICAL SECTION (at 1 Wimpole Street, London, W.1), at 2.30 p.m.—Dr. W. R. Bett: "On Style in Medical Literature—Traditions, Circumstances, Diversions and Lethargies".

INSTITUTION OF ELECTRICAL ENGINEERS, RADIO SECTION (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. C. Domb and Dr. M. H. L. Pryce: "The Calculation of Field Strengths over a Spherical Earth".

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 6 p.m.—Scientific Papers.

Thursday, February 6

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 11 a.m.—Conference on "Proposed International Exhibition in 1951".

ROYAL SOCIETY OF ARTS, INDIA AND BURMA SECTION (at John Adam Street, Adelphi, London, W.C.2), at 2.30 p.m.—Sir Herbert Howard: "The Forests and Forest Resources of India".

LINNEAN SOCIETY OF LONDON (joint meeting with the SYSTEMATICS ASSOCIATION, at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Discussion on "The Teaching of Taxonomy" (Speakers: Sir Clive Forster-Cooper, F.R.S., Prof. James Gray, F.R.S., Dr. W. B. Turrill and Prof. A. R. Clapham).

INSTITUTION OF ELECTRICAL ENGINEERS (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. J. Hacking and Mr. J. D. Peattie: "The British Grid System in Wartime".

INSTITUTE OF ECONOMIC ENGINEERING, MIDLAND SECTION (at the Chamber of Commerce, Birmingham), at 7 p.m.—Mr. C. D. Pollard: "Time and Motion Study in the Foundry".

CHEMICAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 7.30 p.m.—Dr. A. E. Alexander: "The Application of Surface Chemistry to Colloidal Problems" (Tilden Lecture).

Friday, February 7

INSTITUTION OF ELECTRICAL ENGINEERS, MEASUREMENTS SECTION (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Discussion on "What has the Electrical Instrument Industry to learn from Developments in Continental Practice?" (to be opened by Mr. F. E. J. Ockenden and Mr. G. N. Harding).

INSTITUTION OF MECHANICAL ENGINEERS, APPLIED MECHANICS GROUP (at Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Prof. A. Porter: "Basic Principles of Automatic Control Systems".

GEOLOGISTS' ASSOCIATION (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 6 p.m.—Dr. J. M. Bruckshaw: "Application of Geophysics to Geological Problems".

CHEMICAL SOCIETY (joint meeting with the LOCAL SECTIONS OF THE ROYAL INSTITUTE OF CHEMISTRY and the SOCIETY OF CHEMICAL INDUSTRY, at University College, Cardiff), at 6.45 p.m.—Prof. W. Wardlaw: "Structural Inorganic Chemistry".

TEXTILE INSTITUTE, DUBLIN BRANCH (at the Mansion House, Dawson Street, Dublin), at 7.30 p.m.—Mr. E. Waters: "Nylon Textiles—their Properties and Processing".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 9 p.m.—Prof. James Gray, F.R.S.: "Vitalism, Old and New".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

GAS EXAMINER—The Town Clerk, Municipal Offices, Bromley, Kent, endorsed 'Gas Examiner' (February 7).

FARM MANAGER—The Director, Agricultural Research Council, Field Station, Compton, Berks. (February 10).

HEAD OF THE ESTABLISHMENT, Survey Production Centre, R.E., Hanwell, under the War Office—The Secretary, Civil Service Commission, 6 Burlington Gardens, London, W.1, quoting No. 1777 (February 15).

JUNIOR CHEMIST and a JUNIOR PHYSICIST—The Director, Institute for Industrial Research and Standards, 45 St. Stephen's Green, Dublin (February 15).

ASSISTANT SUPERINTENDENT (Research), Government Livestock Farm, Hissar, in the Punjab Veterinary Service, Class I—The High Commissioner for India, General Department, India House, Aldwych, London, W.C.2, quoting No. 306 (February 15).

GEOLOGIST for the Government of the United Provinces, India—The High Commissioner for India, General Department, India House, Aldwych, London, W.C.2, quoting No. 298 (February 15).

CLINICAL CHEMIST to be responsible for tests and investigations of a chemical nature in the Clinical Pathology Department—The Secretary, University College Hospital Medical School, University Street, London, W.C.1 (February 28).

SENIOR LECTURER (Grade I) in the Department of Electrical Engineering (Electronics)—The Registrar, The University, Liverpool (February 28).

READERSHIP IN PHYSICAL CHEMISTRY tenable at the Imperial College of Science and Technology—The Academic Registrar, University of London, Senate House, London, W.C.1 (March 4).

SENIOR LECTURER IN ANATOMY (Histology)—The Registrar, The University, Sheffield (March 8).

CHAIR OF CHINESE ART AND ARCHÆOLOGY tenable at the Courtauld Institute of Art—The Academic Registrar, University of London, Senate House, London, W.C.1 (March 11).

OFFICIAL FELLOWSHIP IN NATURAL SCIENCES OR MEDICINE—The Master, St. Catharine's College, Cambridge (March 31).

LECTURER (Grade IIb) IN MECHANICAL ENGINEERING—The Secretary, The University, Edmund Street, Birmingham 3 (March 31).

SENIOR LECTURER IN ANATOMY—The Registrar, The University, Sheffield (April 1).

SENIOR LECTURER IN ZOOLOGY—The Registrar, The University, Sydney, Australia (April 5).

SPEECH THERAPIST—The County Medical Officer, County Hall, Hertford.

LECTURER IN PHYSICS (with subsidiary Mathematics)—The Registrar, Merchant Venturers' Technical College, Unity Street, Bristol 1.

LECTURER IN CHEMISTRY, a LECTURER IN BOTANY, and an ASSISTANT LECTURER IN SCIENCE SUBJECTS—The Principal, Kingston Technical College, Kingston Hall Road, Kingston-upon-Thames, Surrey.

BIOLOGIST on the staff in Tanganyika Territory—The Secretary, Empire Cotton Growing Corporation, 1a Harrington Road, London, S.W.7.

SENIOR BACTERIOLOGIST to the Royal Sheffield Infirmary and Hospital and Honorary Lecturer in the Bacteriology Department of the University of Sheffield—The General Superintendent, Royal Infirmary, Sheffield 6.

LECTURER IN GEOLOGY, an ASSISTANT LECTURER IN HORTICULTURE, a LECTURER IN SOCIAL PHILOSOPHY, and a LIBRARY ASSISTANT—The Registrar, University College, Exeter.

SCIENTIFIC LIAISON OFFICER, and a TECHNICAL LIAISON OFFICER, in the Department of Scientific and Technical Liaison in the London Offices of the Secretariat—The Secretary, International Wool Secretariat, Dorland House, 18–20 Regent Street, London, S.W.1.

JUNIOR RESEARCH OFFICER (with honours degree in Physics or Engineering) for research on the mechanics of cloth formation, and a RESEARCH OFFICER (male) for research on development and application of analytical methods—The Director of Research, British Cotton Industry Research Association, Shirley Institute, Didsbury, Manchester 20.

HYDROGRAPHICAL SURVEYORS (Grade III), by the Government of Iraq for the Basrah Port Directorate—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.N.17048.

REPORTS and other PUBLICATIONS

(*not included in the monthly Books Supplement*)

Great Britain and Ireland

British Standard 1270 : 1946. Schedule for Electric Discharge Lamps for General Purposes. Pp. 8. (London : British Standards Institution, 1946.) 2*s*. net. [1010

Imperial Bureau of Horticulture and Plantation Crops. Technical Communication No. 18 : Fruit Fall and its Control by Synthetic Growth Substances. By Dr. M. C. Vyvyan. Pp. 72. (East Malling : Imperial Bureau of Horticulture and Plantation Crops, 1946.) 3*s*. 6*d*. [1410

Edinburgh and East of Scotland College of Agriculture. Technical Bulletin No. 1 : Investigations on Bovine Mastitis, 1, Bacteriological Diagnosis by Plating Methods. By A. Cunningham, Anna C. Stirling, Joan K. B. Raymont and Margaret A. Wilson. Pp. 20 + 2 plates. (Edinburgh : Edinburgh and East of Scotland College of Agriculture, 1946.) [1410

The Registrar-General's Statistical Review of England and Wales for the Year 1942. Tables, Part 1 : Medical. Pp. vii + 320. (London : H.M. Stationery Office, 1946.) 5*s*. net. [1410

Forestry Commission. Bulletin No. 8 : British Bark-Beetles. By Dr. J. W. Munro. Reprint. Pp. 77 + 10 plates. (London : H.M. Stationery Office, 1946.) 3*s*. 6*d*. net. [1410

Ministry of Agriculture and Fisheries. Bulletin No. 109 : Commercial Flower Production, Part 2, Summer Flowers. By Dr. H. V. Taylor and Dr. Katharine H. Johnstone. Reprint of 1939 edition. Pp. vi + 62 + 10 plates. 1*s*. 6*d*. net. Bulletin No. 110 : Commercial Flower Production, Part 3, Foliage and Foliage Plants. By Dr. H. V. Taylor and Dr. Katharine H. Johnstone. Reprint of 1938 edition. Pp. iv + 56. 1*s*. net. Bulletin No. 112 : Commercial Flower Production, Part 4, Flowering Plants in Pots. By Dr. H. V. Taylor and Dr. Katharine H. Johnstone. Reprint of 1939 edition. Pp. vi + 56 + 12 plates. 2*s*. net. (London : H.M. Stationery Office, 1946.) [1410

Overhead-Line Charts : a Series of Charts giving Load-Carrying Capacity and Voltage Drop for Line Voltages of from 400V to 264kV. By J. S. Forrest. Pp. 20. (London : *Electrical Review*, Ltd., 1946.) 2*s*. 6*d*. [1510

Apparent Places of Fundamental Stars, 1946 : containing the 1535 Stars in the Third Fundamental Catalogue (FK3) of the Berliner Jahrbuch. (Published by order of the Lords Commissioners of the Admiralty.) Pp. xxxii + 538. (London : H.M. Stationery Office, 1945.) 42*s*. net. [1510

Research Association of British Rubber Manufacturers. Twenty-sixth Annual Report for the Year 1945. Pp. 37. (Croydon : Research Association of British Rubber Manufacturers, 1946.) [2110

Ministry of Fuel and Power and the Department of Scientific and Industrial Research. Domestic Heating in America : a Study of Heating, Cooking and Hot Water Supply in Small Houses in U.S.A. and Canada. Pp. x + 152. (London : H.M. Stationery Office, 1946.) 3*s*. net [2110

Forestry Commission. Bulletin No. 14 : Forestry Practice ; Summary of Methods of Establishing Forest Nurseries and Plantations with Advice on other Forestry Questions, for Owners, Agents and Foresters. Revised edition. Pp. v + 92. (London : H.M. Stationery Office, 1946.) 2*s*. net. [21

Other Countries

Scientific Reports of the Imperial Agricultural Research Institute, New Delhi, for the Year ended 30th June 1945. Pp. 93. (Delhi : Manager of Publications, 1946.) 1.10 rupees. [24

Indian Forest Records (New Series). Silviculture, Vol. 6, No. 1 : An English Translation of the Results of an Erosion Experiment published in the Proceedings of the Swiss Forest Research Institute, Vol. 23, Part 1, 1943, by Hans Burger. Translation by Dr. A. Griffith and C. W. A. Soudermann. Pp. vi + 61 + 3 plates. (Dehra Dun : Forest Research Institute, 1946.) 1.11 rupees. [24

Commonwealth of Australia : Council for Scientific and Industrial Research. Bulletin No. 194 : A Procedure of Investigation in Fisheries Biology. (Division of Fisheries, Report No. 11.) By G. L. Kesteven. Pp. 31. Industrial Chemistry Circular No. 5 : The Significance, Measurement and Control of Hydrogen Ion Concentration. By D. Allan Walkley. Pp. 30. (Melbourne : Government Printer, 1946.) [28

South Australia : Department of Mines. Mining Review for the Half-Year ended 30th June, 1945. (No. 82.) Pp. 133. (Adelaide : Government Printer, 1946.) [28

Transactions of the American Philosophical Society. New Series, Vol. 35, Part 4 : A Census of the Determinable Genera of the Stegocephalia. By Prof. E. C. Case. Pp. 323-420. (Philadelphia : American Philosophical Society, 1946.) [28

West China Union University Museum. Guidebook Series No. 6 : An Introduction to Tibetan Culture. By Cheng Te-k'un and D. Michael Sullivan. Pp. 40. (Chengtu : West China Union University Museum, Hwasipa, 1945.) [28

Annals of the New York Academy of Sciences. Vol. 46, Art. 5 : Lymph. By Philip D. McMaster, Robert Chambers, Elliot R. Clark, Thomas F. Dougherty, Cecil K. Drinker, William E. Ehrich, Eugene M. Landis, Valy Menkin, Paul A. Nicoll, Richard L. Webb, Abraham White and B. W. Zweifach. Pp. 679–882 + 14 plates. (New York : New York Academy of Sciences, 1946.) [28

Kungl. Lantbrukshögskolans Annaler (Annals of the Royal Agricultural College of Sweden.) Vol. 13. Pp. v + 296. (Uppsala : Kungl. Lantbrukshögskolan, 1946.) 15 kr. [28

Ministry of Public Works, Egypt. Physical Department Paper No. 52 : Earth and Water Temperatures in Egypt. By L. J. Sutton. Pp. v + 91. (Cairo : Government Press, 1946.) [29

Fouad I University : Faculty of Science. Annual Report, 1944–1945. Pp. v + 28. (Cairo : Fouad I University Press, 1946.) [29

Western Australia : Department of Mines. Mineral Resources of Western Australia, Bulletin No. 1 : Census of Western Australian Minerals. By Dr. Dorothy Carroll. Pp. 72. (Perth : Government Printer, 1945.) [30

Banana Leaf Spot (*Mycosphaerella musicola*) on the Gros Michel Variety in Jamaica : Investigations on the Aetiology of the Disease and the Principles of Control by Spraying. By R. Leach. Pp. 118 + 7 plates. (Kingston : Government Printer, 1946.) 2*s*. [30

Meddelande från Lunds Astronomiska Observatorium. Ser. 2, Nr. 113 : A Photometric and Statistical Investigation of 1119 Stars in the Hyades Region. By Erik Holmberg. Pp. 107. Ser. 2, Nr. 114 : A Photographic Photometry of the Spiral Nebulae NGC 891 and NGC 3623. By Erik Holmberg. Pp. 27. Ser. 2, Nr. 115 (Historical Notes and Papers Nr. 20) : Alt-Uppsala und Urnes ; Untersuchungen zur Entstehung der Dreischiffstabkirche und des ältesten Germanisch-Heidnischen Kulthauses. Von Åke Ohlmarks. Pp. 30. Ser. 2, Nr. 116 : A Study of Visual Binary Stars, with regard to the Errors of Observation and the Problem of Orbit Determination. By Bengt Ekenberg. Pp. 190. 10 kr. Ser. 2, Nr. 117 : On the Apparent Diameter and the Orientation in Space of Extragalactic Nebulae. By Erik Holmberg. Pp. 82. Ser. 2, Nr. 118 : Proper Motions of 98 Red Variable Stars. By Frida Palmér. Pp. 10. Ser. 2, Nr. 119 : Studies of Bright Diffuse Galactic Nebulae, with special regard to their Spatial Distribution. By Sven Cederblad. Pp. 116. 10 kr. (Lund : Astronomical Observatorium, 1944–1946.) [30

Meddelande från Lunds Astronomiska Observatorium. Ser. 1, Nr. 106 : Untersuchungen über den Konstanzbereich von Zählrohren und das Auflösungsvermögen von Verstärkern. Von Karl Erik Forsman. Pp. 12. Ser. 2, Nr. 107 (Historical Notes and Papers Nr. 16) : L'Étude de l'astronomie chez les Grecs au XVIII Siècle. Par D. Kotakis. Pp. 9. Ser. 2, Nr. 108 : Investigations into the Visual Extinction. By Bengt Ekenberg. Pp. 32. 2.25 kr. Ser. 2, Nr. 109 : Meridian Observations of Double Stars in the AG Zone of Lund. By Carl-Gustav Danver. Pp. 23. Ser. 2, Nr. 110 (Historical Notes and Papers Nr. 17) : Stenar, die mythische Bifrost den Regenbogen oder die Milchstrasse darstellen. Von Åke Ohlmarks. Pp. 40. Ser. 2, Nr. 111 (Historical Notes and Papers Nr. 18) : The Rise of Astrology in the Hellenistic Age. By Martin P. Nilsson. Pp. 9. Ser. 2, Nr. 112 (Historical Notes and Papers Nr. 19) : Nicolaus Kopernikus (Kopernik) and his Astronomical Reformation. By Knut Lundmark. Pp. 18. (Lund : Astronomical Observatorium, 1939–1944.) [30

Meddelande från Lunds Astronomiska Observatorium. Ser. 1, Nr. 150 : A Criticism of the Hypothesis of Electrostatic Accumulation of Meteorites. By Conrad Lönnqvist. Pp. 17. Ser. 1, Nr. 154 : On the Interpretation of the Spectroscopically Observed Rotations of Galaxies. By Dr. Erik Holmberg. Pp. 13. Ser. 1, Nr. 155 : Supernovae. By Knut Lundmark. Pp. 17. Ser. 1, Nr. 156 : Estimates of the Total Magnitudes of ζ Tucanae, ω Centauri, M6 and M7. By Bengt Ekenberg. Pp. 4. Ser. 1, Nr. 157 : The Inclination of the Galaxies towards the Tangential Plane. By Carl-Gustav Danver. Pp. 8. Ser. 1, Nr. 158 : On the Photographic Resolution of Galaxies. By Carl-Gustav Danver. Pp. 17. Ser. 1, Nr. 159 : A Note on an Elementary Interpretation of Correlation Formulae. By Anders Reiz. Pp. 5. Ser. 1, Nr. 160 : Polytropic Changes in an Inclosure containing Matter and Radiation. By Anders Reiz. Pp. 4. Ser. 1, Nr. 161 : On the Numerical Solution of certain Types of Integral Equations. By Anders Reiz. Pp. 2. Ser. 1, Nr. 162 : The Systematic Corrections and Weights of the Reobserved AG Zone of Lund. By W. Gyllenberg. Pp. 5. (Lund : Astronomiska Observatorium, 1939–1944.) [30